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IMPURE IMPACT GIVING:
THEORY AND EVIDENCE

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ABSTRACT

We present a new model of charitable giving where individuals regard out-of-pocket donations and the matches they induce as different. We show that match-price elasticities combine conventional price effects with the strength of warm-glow, so that a match-price elasticity alone is insufficient to characterize preferences for giving. Match- and rebate-price elasticities will typically be different, but together they lead to tests of underlying giving preferences. We estimate, for the first time, a match-price elasticity together with a real-world tax-based rebate elasticity in a non-laboratory high-stakes setting. The estimates reject extant models of giving, but are consistent with the new theory.

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1. Introduction

Having a third-party match other donors' voluntary contributions to a public good is a fundraising intervention widely used by charitable organizations. Accordingly, there is a large empirical literature in economics using matches to investigate preferences for giving.[1] The extant theory used by economists to motivate this literature assumes that donors derive utility from the amount of donations received by a charity and that matches lower the price, in terms of forgone consumption, of providing \$1 to a charity (e.g., see Andreoni and Payne, 2013). The results in the empirical literature are fairly uniform: the amounts charities receive are very responsive to matches. The interpretation based on the extant theory is that giving is price elastic, or nearly so.

However, there are unresolved puzzles, both empirical and theoretical, that indicate that this current interpretation cannot be correct. First, as is well-known, the extant theory of matches predicts equivalent responses to a match at rate m and a rebate at rate $t = m/(1 + m)$. Contradicting this prediction are lab experiments (Blumenthal, Kalambokidis, and Turk, 2012; Eckel and Grossman, 2003, 2006a), field experiments (Eckel and Grossman, 2008, 2017), an incentivized survey experiment (Bekkers, 2015) and a hypothetical scenario experiment (Scharf and Smith, 2015) all of which have found that the match-price elasticity and rebate-price elasticity are not even close to each other. If this evidence is valid, then the extant theory is not. Further, while match- and rebate- elasticities differ, the so-called "checkbook" elasticity induced by the match—based on the out-of-pocket checkbook amounts donors write to charities, excluding the match itself—is often inelastic, small, and closer to the rebate elasticity. This raises the question: which elasticity produced by a match, checkbook or amount-received, should we expect to equate with a rebate elasticity?

These issues reflect a more fundamental puzzle concerning what a match-price elasticity is actually telling us about preferences. Suppose a donor is offered a 1-to-1 match and her

[1] See for example Karlan and List (2007), Meier (2007), Huck and Rasul (2011), Meer (2017), Eckel and Grossman (2003, 2008), Davis (2006), Bekkers (2015), Scharf and Smith (2015), and others discussed below. Vesterlund (2016) provides a survey.

checkbook giving does not change at all in response. This scenario is not too dissimilar from the findings in a number of empirical studies. What does this response mean? The interpretation offered by the extant theory is that the price of securing a dollar increase in the charity's receipts was cut in half and the amount of donations received by the charity doubled, so that the donor must have unit-elastic price responsiveness regarding the charity's amount received. An alternative interpretation is that the donor cares only about her checkbook donation: in this case the match did not alter her incentives in any way, so her checkbook amount did not change. These are extremely different descriptions of donor preferences. But prior research has not determined how to differentiate between them.

This paper makes a theoretical and an empirical contribution towards the understanding of donor preferences, matches, and incentives to give. First, we present a theoretical explanation of the above puzzles based on a long-established construct in the economics of charitable giving: warm-glow. In the new theory, a donor gets utility from how much her gift increases the amount received by the charity—the *impact* of her gift (Duncan, 2004; see also Atkinson, 2009)—just as in the extant theory. The innovation is that she also gets *warm-glow* utility (Andreoni, 1989) from her donation. Because the new theory combines impact with warm-glow, we call it "impure impact giving".

In the model, a change in the match rate changes the opportunity cost of impact, in terms of forgone own consumption, but does *not* change the opportunity cost of warm-glow. In contrast, a rebate changes the opportunity cost of both. The impure impact model thus predicts non-equivalence between the match-price elasticity and the rebate-price elasticity. We show that the rebate effect can be thought of as a "conventional" price effect in that it can be decomposed into traditional Slutsky substitution and income effects. In contrast, the match effect depends not only on the conventional price effect but also upon the strength of warm-glow. Specifically, warm-glow alters both the substitution effect and the income effect, making the former weaker and the latter stronger. The theoretical representation of the altering of the income effect is equivalent to Andreoni's (1989) analysis of how warm-glow alters crowding-out of government grants. Because warm-glow changes the substitution effect and the income effect in opposing directions, the net effect of warm-glow on the match elasticity depends upon which change dominates.

Analysis of the model provides further insights about the other puzzles mentioned above. Which of the two match-price elasticities, checkbook or amount-received, should one expect to be conceptually the same as the rebate elasticity? The answer is neither. Then, having observed a small checkbook response to a match, what should one conclude about preferences? The answer is that the match-price elasticity alone is not sufficiently informative. In an impure impact model *both* rebate-price and match-price elasticities are necessary to make inferences about the nature of preferences.

Because the impure impact model nests canonical models of charitable giving—the pure impact model (Duncan, 2004; Atkinson, 2009), pure warm-glow, pure altruism (Becker, 1974) and impure altruism (Andreoni, 1989)—it provides a framework within which the match and rebate elasticities together can be used to investigate the nature of preferences. Our analysis of the impure impact model leads to tests for these models based on the two elasticities. Observing just one of the elasticities is generally not enough to conduct the tests. But as pointed out by Meer (2014), observing both of these elasticities together is typically not feasible because the same data rarely afford estimation of multiple elasticities in parallel.

Our second contribution is to provide parallel estimates of match and rebate elasticities generated from a match and a real-world tax rebate. The estimates come from donations to a university over a twelve-year period. During a 19-month window within this period, a 1-to-1 match was made available to a subset of donors. The match turned on and off, motivating a difference-in-differences approach to estimating the match-price elasticity. The baseline estimate of this elasticity is -1.2, qualitatively similar to results from the matching literature.

During the entire twelve-year period the donors living in the same state as the university were eligible for a 50 percent state tax credit for donations to universities. The credit was capped at \$400, causing excess bunching of donations at the cap and motivating a bunching-based approach. We develop two new bunching estimators based on identifying assumptions weaker than those used in standard bunching estimators (although we also apply the standard estimators developed by Saez, 2010, and Kleven and Waseem, 2013). One of the new estimators is similar in spirit to that proposed by Blomquist and Newey (2017). Both of the new estimators use identifying assumptions different from each other (and different from the standard estimators), but both produce similar rebate-price elasticity estimates of $\approx -.2$ (as do the standard estimators). Hence, the match and rebate elasticity estimates are qualitatively similar to the results from the lab, field, survey, and hypothetical-scenario experiments: the match-price elasticity is elastic, the rebate-price elasticity is inelastic and small, and the extant theory is not compatible with donor behavior. This is the first evidence of non-equivalence from parallel estimation of responses to a match and a real-world tax rebate.

The results have several additional implications. First, the findings indicate that future work on match prices should consider how to estimate a conventional price effect in parallel with estimating a match-price effect. If all one has is an estimate of the match-price elasticity, there is an identification problem: the conventional price effect and the strength of warm-glow will be confounded.

Next, in the standard impure altruism model interventions to increase the public good are more effective in the presence of warm-glow (Andreoni, 1989). However, in an impure impact model it is possible that the presence of warm-glow makes interventions *less* effective. We discuss scenarios where this is possible and provide a simple example.

Third, the empirical tests derived from the theory not only reject the extant pure impact model but also reject pure warm-glow. The rejection of pure warm-glow is noteworthy because the empirical setting is one with many donors and a large amount being given to the public good. In such a setting, asymptotic analysis of impure altruism indicates that preferences on the margin should be almost entirely influenced by their warm-glow component (Ribar and Wilhelm, 2002). But despite the many donors/large-public-good setting, the results indicate donors continue to get utility from the amount received by the charity. That, combined with the rejection that the amount received is all they get utility from, is consistent with the impure impact model. We also combine the empirical and theoretical results to generate estimates of the compensated price elasticity of giving. These estimates are close to the observed rebate elasticity, and far from the observed match elasticity.

Finally, it is well-known that because rebate elasticities are inelastic, the amount received by charities can be much higher if the price reduction is introduced via a match rather than via a rebate (e.g., Turk et al., 2007; Eckel and Grossman, 2017). Hence, charities prefer matches to rebates. In contrast, we show that *donor* welfare is higher if price reductions are introduced via a rebate. The preference of donors for rebates should be included in normative analyses of incentives to give.

The rest of the paper is organized as follows. Section 2 presents the impure impact model, proves the non-equivalence result, derives the Slutsky decompositions, discusses implications of impure impact for understanding puzzles in the matching literature, discusses testing, and proves the normative result. Section 3 describes the matching environment, the tax credit policy, and the data, and also the difference-in-differences and bunching methods. Section 4 carries out the estimation and robustness checks. Section 5 provides a general discussion of the theoretical and empirical results, including the connection with results from several other papers in the matching literature. Section 6 concludes.

2. Theory

2A. Pure impact

Before turning to the model of impure impact, first consider the baseline model that motivates the matching literature. Suppose a donor i makes a gift g_i to a charity; the gift is matched by a third party at rate m so that the charity receives the amount $R_i = (1+m)\, g_i$, and the donor gets utility $U(c_i,\ R_i)$ from own consumption c_i and R_i. This model is Duncan's (2004) pure impact theory of philanthropy: R_i is the impact i has on the charity's output. Atkinson (2009) proposes a similar model. In the matching literature R_i is called the "amount received", and g_i is called the "checkbook amount".

The budget constraint is $c_i + g_i = y_i$ where y_i is the donor's income. To facilitate the proof of match- and rebate-price equivalence, introduce a rebate at rate t for each dollar of checkbook giving. Such rebates typically come from the tax code, so we refer to the rebate incentive as the "tax-price". Then the budget constraint is $c_i + g_i = y_i + t\, g_i$. Bringing the rebate term to the left-hand side and substituting $g_i = R_i/(1+m)$ produces a constraint in terms of the amount received: $c_i + p_m\, p_t\, R_i = y_i$, where the match-price is $p_m = 1/(1+m)$ and the tax-price is $p_t = (1-t)$. To maintain parallel treatment of the match- and tax- price, we take the sources of funds that finance both to be exogenous to the donor.

Substituting the budget constraint into utility $U(y_i - p_m\, p_t\, R_i,\ R_i)$, and maximizing with respect to R_i yields the first-order condition:

$$- p_m\, p_t\, U_c(y_i - p_m\, p_t\, R_i,\ R_i) + U_R(\cdot,\cdot) = 0 \quad (1)$$

which can be solved for the optimal amount received:

$$R_i^* = q(y_i,\ p_m\, p_t). \quad (2)$$

Equation (2) is a demand function in terms of income and price. If the rebate is designed to be "price-equivalent" $t = m/(1+m) \Rightarrow p_t = p_m$, then it follows immediately that R_i^* under a match will be equivalent to R_i^* under a rebate. Further, $\frac{\partial R_i^*}{\partial p_m} = \frac{\partial R_i^*}{\partial p_t}$, implying equivalence of the respective elasticities: $e_m = e_t$.

The optimal checkbook amount follows from (2): $g_i^* = p_m\ \ q(y_i,\ p_m\, p_t)$. The checkbook elasticity $e_{m,b}$ reflects the mechanical relationship between the amount received and the checkbook amount donated: $e_{m,b} = 1 + e_m$. Changing the match rate m both changes the price p_m and, for a given checkbook donation, mechanically changes the amount received by the charity.

2B. Impure impact giving

In the impure impact model the donor also gets warm-glow utility from her checkbook giving: $U(c_i,\ g_i,\ R)$. In this utility function $R \equiv R_i + \lambda\ R_{-i}$, where $R_i = (1+m)g_i$ as before, R_{-i} is exogenous output contributed by others, and λ is a weight. R_{-i} models challenge grants or lead donations that are exogenous to i's checkbook giving (e.g., Rondeau and List, 2008; Huck and Rasul, 2011). The weight λ introduces flexibility in how i regards the exogenously contributed output relative to her own impact R_i. If $\lambda = 1$, the two are viewed as perfect substitutes, and if $p_m = 1$ also, then the model becomes the standard impure altruism model; removing the warm-glow term in this case produces the pure altruism model. If $\lambda < 1$, donor i regards her own impact more highly than that of others; the case of $\lambda = 0$ would imply that she disregards exogenous output entirely and the model becomes a model of impact and warm-glow alone. Removing the warm-glow term in this case would produce the pure impact model. The weight λ is an unobservable preference parameter, so that R is an unobservable function of the observables R_i and R_{-i}. This does not present a problem for the analysis of the response of observed R_i to price because given the above definitions $\frac{\partial R_i}{\partial p} = \frac{\partial R}{\partial p}$ where p represents p_m or p_t. One could further generalize preferences to be $U(c_i,\ g_i,\ R_i,\ R_{-i})$; doing so would not change the main findings and in Appendix A.3 we derive the results of this section using these more general preferences. The preferences analyzed here are sufficiently general to nest the standard models while allowing a succinct presentation of the results.

The budget constraint is the same as in Section 2A, $c_i\ +\ p_m\, p_t\, R_i = y_i - \tau_i$, except we now include a lump-sum tax τ_i. Add $p_m\ p_t\ \lambda\ R_{-i}$ to both sides of the constraint:

$$c_i \ + \ p_m\ p_t\ R \ = \ Z_i \ \equiv \ y_i \ - \ \tau_i \ + \ p_m\ p_t\ \lambda\ R_{-i} \tag{3}$$

where Z_i is social income. As before $g_i = p_m\ R_i$, but now $R_i = R \ - \ \lambda\ R_{-i}$. Substituting these and the budget constraint into utility:

$$U(Z_i - p_m p_t R,\ p_m(R - \lambda R_{-i}),\ R) \tag{4}$$

and maximizing with respect to R yields the first-order condition:

$$-p_m p_t U_c(Z_i - p_m p_t R,\ p_m(R - \lambda R_{-i}),\ R) \ + \ p_m U_g(\cdot,\cdot,\cdot) \ + \ U_R(\cdot,\cdot,\cdot) \ = \ 0. \tag{5}$$

The marginal utilities in the first order condition depend on four arguments: social income Z_i, weighted exogenous output $p_m\ \lambda\ R_{-i}$, a price term $p_m\ p_t$ that also appeared in the pure impact demand function (2), and a second price term p_m. Each of these arguments are exogenous to i. Accordingly, (5) can be solved for the optimal impact as a function of these four:

$$R^* \ = \ q(Z_i,\ \lambda\ p_m\ R_{-i},\ p_m\ p_t,\ p_m). \tag{6}$$

Equation (6) is the impure impact demand curve for R. Note that the exogenous output term $p_m\ \lambda\ R_{-i}$ enters $q(\cdot)$ on its own (in addition to its entering through social income Z_i) only because of the warm-glow term in utility (4). Likewise, the price term p_m enters $q(\cdot)$ on its own in the fourth argument only because of warm-glow.

Denote q_1 as the partial of $q(\cdot)$ with respect to Z_i. Then using equation (3) the effect of own income on demand is $\frac{\partial R^*}{\partial y_i} = q_1$. The effect of exogenous output is $\frac{\partial R^*}{\partial(\lambda p_m R_{-i})} = p_t q_1 + q_2$. Because $g_i^* = p_m\ R_i^* = p_m\ (R^* - \lambda R_{-i})$, we have $\frac{\partial g_i^*}{\partial(\lambda p_m R_{-i})} = p_m(p_t q_1 + q_2) - 1$. The expression $p_m(p_t q_1 + q_2) - 1$ is how much an increase in (λ-weighted) exogenous output crowds-out donor i's checkbook amount g_i. The presence of warm-glow $q_2 > 0$ implies a mitigation of crowd-out: $p_m(p_t q_1 + q_2) - 1$ is closer to zero. These relationships were first explained by Andreoni (1989) in the context of the impure altruism model (in which $\lambda = 1$ and $p_m = 1$) without a tax rebate ($p_t = 1$). An equivalent way to think about warm-glow's mitigation of crowd-out is that warm-glow enhances the $\frac{\partial R^*}{\partial(\lambda p_m R_{-i})}$ "income effect"; q_2 is the difference between this income effect and the own-income effect (Ottoni-Wilhelm, Vesterlund, and Xie, 2017).[2]

The partial derivative q_3 is the price effect that is the counterpart to the price effect that appeared in the pure impact version of the model, equation (2). Suppose that p_t is reduced by a penny, holding the other terms in (6) constant. This reduces the opportunity cost of *both* one's checkbook donation g_i and one's impact R_i. In contrast, if there is a match ($p_m < 1$), and i sacrifices one dollar of own consumption c_i to increase her checkbook donation, she does *not* get $1/p_m$ units of both R_i and g_i: instead she gets $1/p_m > 1$ units of R_i but only one unit of g_i. Hence the two prices have different effects; the difference is captured by the fourth partial, q_4. The comparative statics with respect to p_t and p_m are:

$$\frac{\partial R^*}{\partial p_t} = p_m \left(\frac{-U_c}{\Delta} - q_1 R_i \right) \tag{7}$$

$$\frac{\partial R^*}{\partial p_m} = p_t \left(\frac{-U_c}{\Delta} - q_1 R_i \right) + \left(\frac{U_g}{\Delta} - q_2 R_i \right) \tag{8}$$

where Δ is the negative of the terms in the second-order sufficient condition; hence $\Delta > 0$ (see Appendix A.1 for derivations of equations (7) and (8)).

Recalling that $\frac{\partial R_i}{\partial p} = \frac{\partial R}{\partial p}$, equations (7) and (8) can be used to derive elasticities of observed impact R_i with respect to each price change, $e_t = \frac{\partial R_i}{\partial p_t}\frac{p_t}{R_i}$ and $e_m = \frac{\partial R_i}{\partial p_m}\frac{p_m}{R_i}$:

[2] In a pure altruism model ($\lambda = 1$ and $p_m = 1$), there is no warm-glow, $q_2 = 0$, and the crowd-out of exogenous output is $-1 + p_t q_1$. If in the pure altruism model the increase in output is endogenously financed by a lump-sum tax ($\mathrm{d}\tau_i = p_t \, \mathrm{d}(\lambda p_m R_{-i})$) so that Z_i does not change, then crowd-out is -1 (see Andreoni, 1989). These results hold in the pure impact model, noting the minor change that crowd-out is in terms of expenditure on R^* (rather than in terms of R^* itself).

$$e_t \quad = \quad p_m \, p_t \Big(- \frac{U_c}{R_i \, \Delta} \; - \; q_1 \Big) \tag{9}$$

$$e_m \quad = \quad e_t \; + \; p_m \, \Big(\frac{U_g}{R_i \, \Delta} \; - \; q_2 \Big). \tag{10}$$

The two terms in the brackets on the right-hand side of (10) are driven by warm-glow: $U_g > 0$ and $q_2 > 0$. In a pure impact model there is no warm-glow, $U_g = 0$ and $q_2 = 0$, and $e_m = e_t$. In an impure impact model, however, there is warm-glow, these terms will not be zero, and non-equivalence of the two elasticities is established.

Using equations (9) and (10), we next recast these price effects as Slutsky decompositions to clarify how and why rebates and matches are different. We then discuss implications of impure impact for understanding puzzles in the matching literature, its implications for empirical testing of several models of giving, and its implications for normative analysis.

The Slutsky decompositions are derived in Appendix A.2. The compensated price elasticity of R_i with respect to p_t is:

$$e_t^H = \frac{-p_m \; p_t \; U_c}{R_i \; \Delta} \tag{11}$$

which leads to a conventional Slutsky decomposition of the rebate-price elasticity in (9):

$$e_t \; = \; e_t^H \; - \; b_{R_i} \; e_y \tag{12}$$

where $b_{R_i} = p_m p_t R_i / y_i$ is the expenditure share spent on R_i, and $e_y = q_1 y_i / R_i$ is the own-income elasticity of R_i. We say this is a "conventional" Slutsky decomposition because estimates of e_t and the income elasticity, along with the observed expenditure share, are sufficient to uncover the compensated elasticity e_t^H, an important element in calculations of welfare effects of taxation.

In contrast, the Slutsky decomposition with respect to the match-price is:

$$e_m \;=\; e_m^H \;-\; \tilde{b}_{R_i}\; e_y \tag{13}$$

where e_m^H is the compensated match-price elasticity and e_y is as before. However, the budget-share $\tilde{b}_{R_i}$ is an expression that depends on an unobserved ratio of marginal utilities: $\tilde{b}_{R_i} = \tilde{p}R_i/y_i$ where $\tilde{p} \equiv p_m(p_t - \frac{U_g}{U_c})$. Therefore estimates of e_m and the own-income elasticity are not sufficient to uncover the compensated elasticity e_m^H. The implication of the Slutsky decompositions (12) and (13) for empirical work is that tax-price estimates can be used to uncover compensated demand, but match-price estimates cannot.[3]

To see the intuition of how warm-glow causes e_m-e_t non-equivalence, substitute (11) into (9) to re-write the Slutsky decomposition of the rebate elasticity as:

$$e_t \;=\; e_t^H \;-\; q_1\; p_m\; p_t. \tag{14}$$

Then substitute (14) into (10) to re-write the match-price elasticity as:

$$e_m \;=\; e_t^H \;+\; \frac{p_m\; U_g}{R_i\; \Delta} \;-\; p_m\; (p_t\; q_1 \;+\; q_2). \tag{15}$$

Comparing equation (15) to the Slutsky decomposition of the rebate elasticity in (14) reveals that warm-glow has two effects that cause the match-price response to depart from the tax-price response. First, the presence of warm-glow mitigates the substitution response to a match e_t^H. The reason is that, unlike a one penny decrease in the tax-price, a one penny decrease in the match-price does not reduce the opportunity cost of warm-glow. The term $\frac{p_m\; U_g}{R_i\; \Delta} > 0$ in (15) captures this mitigation of the substitution effect; mitigation is proportional to the marginal utility of warm-glow. In the extreme case of pure warm-glow, $U_R = 0$ and

[3]The match-price compensated demand is also unconventional in that it depends on the unobserved ratio $\frac{U_g}{U_c}$, as well as the income effects q_1 and q_2 (see Appendix A.2). In the pure impact model there is no warm-glow, $U_g = 0$, $\tilde{p} = p_m p_t$, and the Slutsky decomposition in (13) becomes the same as (12).

the first-order condition (5) implies that the mitigation of the substitution effect is complete: $e_t^H \ + \ \frac{p_m \ U_g}{R_i \ \Delta} = 0$.

Second, warm-glow enhances the income effect by $q_2 > 0$. The third-party match exogenously increases the net amount received by the charity, and the donor's response to this change will be determined by their response to an increase in social income Z_i (hence the q_1 term in (15)) and by their warm-glow preferences (hence the q_2 term). The $q_2 > 0$ term captures how much i disregards the exogenous increase in the net amount received, and accordingly does not crowd it out. To fix this idea, use the mechanical relationship $e_{m,b} = 1 + e_m$ from Section 2A to re-write (15) in terms of the checkbook elasticity:

$$e_{m,b} \ = \ e_t^H \ + \ \frac{p_m \ U_g}{R_i \ \Delta} \ - \ [-1 \ + \ p_m \ (p_t \ q_1 \ + \ q_2)]. \tag{16}$$

The term in square brackets is crowd-out. Warm-glow $q_2 > 0$ mitigates crowd-out (pushing it toward zero), implying *less* of a reduction in the checkbook amount than would be the case in the absence of warm-glow.

Equation (16) thus provides a way to think through the "small-checkbook-elasticity-in-response-to-a-match" puzzle: Is it driven by a near-unit price elasticity of the amount received, or by warm-glow? The answer is potentially either: it could be that warm-glow is weak, in which case (16) reduces to $e_{m,b} \approx e_t^H - p_m p_t q_1 + 1 = e_t + 1$ (the last equality using (14)). Then, if $e_t \approx -1$, $e_{m,b}$ would be ≈ 0. Or, it could be that warm-glow is strong—$\frac{p_m U_g}{R_i \Delta} \approx |e_t^H|$ and $p_m(p_t q_1 + q_2) \approx 1$—so (16) reduces to $e_{m,b} \approx 0$. Equation (16) makes it clear that these two answers cannot be differentiated if the match-price elasticity is all that is estimated.

More generally, both equation (10) and equation (15) indicate that one cannot observe a match-price elasticity in isolation and separately identify the price elasticity of impact R_i apart from the strength of warm-glow. The match-price elasticity incorporates both effects. Conversely, if all that is observed is an estimate of e_t, one cannot identify what the response to a $m = t/(1-t)$ match would be. The implication of (10) and (15) is that the answer to the question—Which of the two match-price elasticities, amount received or checkbook, should one expect to be conceptually the same as the rebate elasticity?—is neither.

Moreover, because warm-glow's two effects—altering both the substitution effect and the income effect—push the response to a match in opposite directions, whether a match is more effective than a rebate in increasing the amount received depends on the sign of $\frac{U_g}{R_i \Delta} - q_2$. If (and only if) warm-glow's mitigation of the substitution effect is weaker than its income effect ($\frac{U_g}{R_i \Delta} < q_2$), then equation (10) indicates that the presence of warm-glow makes a match more effective in increasing the amount received compared to a rebate. This is how we have become accustomed to thinking about how warm-glow works since Andreoni (1989): the presence of warm-glow makes interventions more effective. However, here it is possible that the presence of warm-glow counter-intuitively makes the intervention *less* effective. In Section 2C we provide an example that illustrates this possibility.

The non-equivalence demonstrated above indicates that the *pure impact* model can be tested straightforwardly: get parallel estimates of e_m and e_t, and test their equality. That is the approach we take in Sections 3 and 4, in a non-experimental setting with a real-world tax policy. Similarly, previous experimental designs also have used two treatments, one to get e_m and the other to get e_t. Alternatively, pure impact could be tested with a single treatment that manipulates both p_m and p_t in different directions so as to hold $p_m \, p_t$ constant. This would be a balanced-budget test of pure impact's assumption that there is no warm-glow.

To test *pure warm-glow*, however, a single match treatment on its own is sufficient. The test is whether $e_m = -1$, and evidence that $e_m \neq -1$ is sufficient to reject pure warm-glow. In contrast, evidence that $e_m \approx -1$ would not be sufficient evidence of pure warm-glow, unless a unit elastic price response $e_t = -1$ could also be ruled out. As we will discuss in Section 5, testing for pure warm-glow has practical significance given the implications of prior studies for our empirical setting.

Finally, we derive a normative implication of impure impact: the presence of warm-glow in the model implies donor welfare is lower under a match compared to a rebate. To prove this, apply the envelope theorem to the indirect utility function from (4), to determine, first, the increase in utility following a reduction in the match-price:

$$\frac{\partial U^*}{\partial p_m} \;=\; (-p_t\, U_c \;+\; U_g)\; R_i \tag{17}$$

and then the increase in utility following a reduction in the tax-price:

$$\frac{\partial U^*}{\partial p_t} \;=\; -\, p_m\, U_c\, R_i. \tag{18}$$

The right-hand side of (18) is unambiguously negative. The first-order condition (5) and $U_R > 0$ imply the right-hand side of (17) is also negative. With $p_m = p_t$ substitute (18) into (17):

$$\frac{\partial U^*}{\partial p_m} \;=\; \frac{\partial U^*}{\partial p_t} + \; U_g\; R_i. \tag{19}$$

The presence of warm-glow $U_g > 0$ implies $|\frac{\partial U^*}{\partial p_m}| < |\frac{\partial U^*}{\partial p_t}|$. A match and rebate both increase donor utility, but the match increases donor utility less. The intuition is that a match reduces the price of impact, but a rebate reduces the price of both impact and warm-glow. The resulting difference in donor utility is proportional to the marginal utility of warm-glow. In contrast, Davis et al. (2006) reason that if checkbook donations are similar under matches and rebates, then rebates are inferior to matches. That argument focuses on the amount of funds received by the charity, but the normative result in (19) suggests that donor welfare should also be considered.

2C. A simple example

We illustrate the impure impact model using a simple example. Consider a quasilinear utility function:

$$U(c_i,\ g_i,\ R_i) \ = \ c_i \ + \ \frac{\theta}{1+\ 1/e} \left(\frac{g_i^{\gamma}\ R_i^{(1-\gamma)}}{\theta}\right)^{1+\ 1/e} \tag{20}$$

in which the impure impact part, $g_i^{\gamma}\ R_i^{(1-\gamma)}$, is Cobb-Douglas. In this model e is the conventional quasilinear price-elasticity parameter, γ is the weight on warm-glow from the checkbook amount g_i relative to impact R_i, and θ is a taste-shifting parameter. Substituting in the budget constraint $c_i = y_i - \tau_i - p_m p_t R_i$ and noting that $g_i = p_m R_i$, it is straightforward to solve for the optimal $R_i{}^*$:

$$R_i{}^* \ = \ \theta\ p_m^{-\gamma} \left(p_m{}^{1-\gamma}\ p_t\right)^e.$$

The rebate- and match-price elasticities are $e_t = e$ and $e_m = -1 + (1-\gamma)(1+e)$. If there is warm-glow, $\gamma \neq 0$, and the two elasticities will be different.

Further, it is clear from the example that observations of both e_m amd e_t can be used to test several cases of the model. If $\gamma \ = \ 1$ the model becomes pure warm-glow and $e_m = -1$, regardless of the value of e. If $\gamma \ = \ 0$ the model becomes pure impact and $e_m = e$, a possibility that cannot be tested for unless both elasticities were observed: if all that is observed is e_m, then all one could say is that is that if the true model is pure impact then the parameter e would equal e_m. But to test the pure impact assumption an independent observation of e is needed, and the rebate elasticity e_t provides that.

The example also illustrates that the rebate elasticity e_t equals the conventional price elasticity e, regardless of the absence or strength of warm-glow. But whether a match is more effective as preferences are characterized by stronger warm-glow depends on e. If e is elastic, $e < -1$ implies $(1+e) < 0$, and then the stronger is warm-glow ($\gamma \uparrow$) the *less* effective is the match: e_m gets pushed closer to -1.

Finally, let V denote the indirect utility function. Then it follows that $\frac{\partial V}{\partial p_m} = (1-\gamma)\frac{\partial V}{\partial p_t}$. Rebates have greater effects on donor welfare than matches, and the difference is greater the stronger is warm-glow.

Overall, both this simple example and the more general analysis of the impure impact model suggest that match- and tax rebate-price elasticities will typically differ, and that estimating both effects is important for testing different motivations to give. A real-world version of that testing requires estimating responses to both a match and a rebate for comparable donors. We turn to this next.

3. Methods for estimating e_m and e_t

3A. The matching environment, the tax credit policy, and the data

There are relatively few studies that compare responses to matches and rebates in the field (Bekkers, 2015; Eckel and Grossman 2008, 2017; Scharf and Smith, 2015; also see Kesternich, Löschel, and Römer, 2016), and none that utilizes variation in a rebate created by tax policy. We will do both. The data are donations made by tens of thousands of people from 2004 to 2015 to a university located in Indiana. The data contain a (scrambled) identifier for each donor, alumni status and graduating class, the date of the donation, the amount, state of residence, and whether the donation was being given jointly with a spouse.

In 2009, a donor from the class of 1960 made a $3 million matching grant to the university to support the Class of 1960 Scholarship Endowment. The grant matched one-to-one donations from members of the 1960 class made in the 19-month period December 1, 2008 through June 30, 2010. This matching environment has features in common with the natural field experiments that have previously estimated match-price elasticities: the matching grant occurred in the field and donors did not know that we would use the data they were generating to estimate their responsiveness to the match. The university development office has confirmed that there were not any other similar large-scale matches made during the data period. Note that because the quality of the university is well-known to its alumni, the offering of a match likely does not function as a quality signal.

The tax policy we investigate is the Indiana Income Tax Credit for Donations to Colleges.

Indiana income taxpayers who make a donation to any higher education institution located in Indiana are eligible for the credit. The credit is a reduction in taxes owed, rather than taxable income, so that (unlike a deduction) the impact of the credit on the price of giving is the same regardless of taxable income.[4] The credit is 50 percent of the donation, up to a maximum donation of $400 for joint filers. We focus on joint filers.[5]

Table 1 provides summary statistics of checkbook donations. In the first four rows the unit of analysis is at the donation-level; this is because estimation of the match-price elasticity uses within-year variation in the availability of the match (as will be discussed in Section 3B below). There are over 8,000 donations from the 1960 class alone. The last four rows present annual donations—all donations made by a donor within a year are added together. Annual donations are the relevant unit of analysis for estimating the response to the tax policy. Across all states, and all years, there are 373,994 annual joint donations (made by 84,295 unique donors), of which 41,129 donations were made by donors residing in Indiana (9,873 donors). In the $200–$1,000 range of giving around the kink there are almost 80,000 annual donations. As reported in the notes to Table 2, there are 7,585 annual joint donations by Indiana residents between $200 and $1000, and the mean of these donations is $383, which is close to the $374 mean among donors in all states (row 8). We use donations in the $200-$1000 range for the baseline kink-based approaches, although results are not sensitive

[4]Donations used for the credit can also be deducted from federal taxable income. However, we show in Appendix A.4 that because the tax credit lowers state income taxes paid, and state income tax is itself deductible from federal taxable income, the combined federal-state price is the product of the federal deduction-price and the state credit-price; the implication is that percentage changes in the state credit-price, and the elasticity estimate based on them, are unaffected by federal deductibility.

[5]We focus on joint donations for two reasons. First, for most of the years of the data (2007 and after) the donation amount needed to enter a lottery for football tickets was $200, exactly the location of the tax credit cap for singles. Hence, for singles, bunching at $200 in response to the tax policy cannot be separated from bunching at $200 to enter the football lottery. The football lottery-kink at $200, however, does not affect the elasticities estimated from joint donations (in Appendix A.8 Tables AT2 and AT3 we redo the estimates for joint donations that did not face this $200 lottery-kink and those estimates are similar to the main estimates). Second, in the data, although "joint" necessarily means "married", "non-joint" can mean either single or that marital status is missing or that the donation was made by a corporation or other legal entity.

to changing the range.[6,7]

3B. Difference-in-differences estimation of the match-price elasticity

We compare the giving behavior of alumni from the 1960 class to the giving behavior of other alumni who were ineligible for the match, before, during, and after the 19-month match period. The approach is difference-in-differences, although unlike standard diff-in-diff the treatment turns both on and off over time. The baseline specification is:

$$\log R_{isctm} \;=\; \delta\, match_{ictm} \;+\; X_{ictm}\, \beta_X \;+\; \phi_c \;+\; \varphi_t \;+\; \lambda_m \;+\; \epsilon \qquad (21)$$

where i lives in state s, is an alumni of class c, and makes the donation in year t, month m. The dependent variable is the logarithm of the amount received $R_i = (1+m)\, g_i$ by the university based on each individual i's checkbook donation g_i. The variable of interest "$match_{ictm}$" is a dummy that equals unity from December 1, 2008 to June 30, 2010, for c = 1960 class, and zero otherwise. The ϕ_c, φ_t, and λ_m represent class, year-of-donation, and month-of-donation dummy variables, capturing variation in donations across classes, trends in donations across years, and seasonal variation within a year.

The match period includes the class of 1960's 50^{th} anniversary, or more specifically, the first six months of the year in which the anniversary occurs. To control for natural increases

[6] The data consist of donations to one university, but an Indiana donor could give to more than one higher education institution. The schedule on which this credit is claimed requires individuals to list the different schools in Indiana that they have donated to. While that information is not publicly available, we have consulted with officials at the Indiana Department of Revenue, and they have told us that the vast majority of credits claimed—on the order of 90 percent—are for donations made to a single school, so that this multiple-school donation concern should not affect the results. Also, because the second of our two new kink methods exploits giving behavior in other states, it should net out any common two-school-donation behavior among donors across states, and results from this method are very similar to the estimates from the other kink methods.

[7] Using the 2005, 2007, and 2009 waves of the *Philanthropy Panel Study*, the generosity module within the *Panel Study of Income Dynamics* (*PSID*), Indiana donors to educational institutions can be placed in the context of general American giving. Indiana residents who donate to education resemble in their overall giving the sample of *PSID* donors who give over \$1,000 per year in total. Average total donations for the former group are \$4,403, and for the latter group are \$4,401. The sample of \$1,000+ per-year donors give about 80 percent of all donations measured in the *PSID*. Below we compare results from our sample to results from other studies.

in donations that occur at significant anniversaries, the X regressors include dummies for 25, 50, and 75 years following a class's graduation. Because the match was available for 13 months prior to the start of the 1960 class's 50^{th} anniversary calendar year and lasted only for the first six months of 2010, we also include in X a "placebo match" variable that switches on 13 months before each class's 50^{th} anniversary begins, and changes back to zero in July of the class's 50^{th} anniversary year. The placebo match thus controls for any tendency for donations from the other classes to increase in the 19-month time period around their 50^{th} anniversaries corresponding to the 19-month match period for the 1960 class.

The coefficient of interest δ represents the percentage change in the amount received in response to the match. $\hat{\delta}$ is converted to a match-price elasticity $\hat{e}_m$:

$$\hat{e}_m = -\frac{\hat{\delta}}{(p_1 - p_m)/\left(\frac{(p_1+p_m)}{2}\right)} \tag{22}$$

where $p_1 = 1$ is the non-match price. Because each observation in this specification corresponds to a separate donation, the estimates are identified off of the intensive margin: the estimated elasticity is the percentage change in the amount received in response to a percentage change in price, conditional on making a donation. To obtain an estimate that also includes the extensive margin, we consider a second dependent variable: the amount received by the university aggregated into class $\times$ state $\times$ month $\times$ year cells (logged). In this case δ is the percentage change in amount received in response to the match and will pick up any effect of the match on the number of checkbook donations being made.

Finally, we check robustness to the inclusion of state dummies, and to the inclusion of a set of interaction state-by-year dummies λ_{st} and month-by-year dummies φ_{tm}; the latter subsume the φ_t and λ_m dummies in (21). The interactions flexibly control for year patterns by state and secular year patterns by month.

3C. Kink-based estimation of the compensated tax-price elasticity

The kink-based estimation approaches we use can be understood from the perspective of two different counterfactuals. The Saez (2010) and Kleven and Waseem (2013) estimators, applied to our setting, are based on an "extended credit" counterfactual where the credit is kept in place but the cap at $400 is removed, thereby extending the credit to larger donated amounts. The first new approach we introduce also is based on this counterfactual. In principle these approaches estimate a compensated tax-price elasticity. The second approach we introduce is based on a different, "unavailable credit" counterfactual—What would happen if the Indiana credit were entirely removed, as is the case in the real-world non-Indiana states?—and in principle estimates an uncompensated tax-price elasticity. We discuss each counterfactual and its accompanying methodology in turn.

To simplify the discussion of the first counterfactual we set aside utility over the charity's exogenous output ($R_{-i} \equiv 0 \Rightarrow R = R_i$), and note that in the absence of matching ($p_m = 1$; $R_i = g_i$) utility can be written in terms of c_i and R_i and the budget constraint (3) becomes $c_i + p_t R_i = y_i - \tau_i$. The tax credit induces $p_t < 1$; however the credit is capped at R_k (e.g., $400), creating a convex kink in the budget constraint as shown in Figure 1. In the first counterfactual where the credit is not capped, the budget line would continue to include the solid line below R_k but would then extend to the dashed straight line above R_k.

Begin with this straight-line budget and imagine the government intervening to introduce the cap: the individual furthest above R_k on the dashed line, who subsequently bunches at R_k after the cap is introduced, is depicted at the equilibrium bundle B. Individual B will have an indifference curve tangent to the upper part of the kink at point K, after the cap is introduced. For individual B, the creation of the kink by capping the tax credit is approximately a compensated price increase, $R_b - R_k \approx R_b - R_c$, if B's income effect is small. Other individuals whose R_i^* would be along the counterfactual dashed budget line $\in (R_k, R_b)$ would also bunch at the kink.

The "bunching interval" $R_b - R_k$ and the price elasticity of R_i can be estimated from the data. Following Saez (2010) we take the representation of preferences in (20), which given the above conditions becomes $U(c_i,\ R_i;\ \theta)\ =\ c_i\ +\ \frac{\theta}{1+1/e}\left(\frac{R_i}{\theta}\right)^{1+1/e}$ where as before e is the price elasticity of R_i and θ is a smoothly-distributed taste parameter describing heterogeneous preferences for R_i.[8] Maximize this utility with respect to $c_i\ +\ p_t\ R_i\ =\ y_i\ -\ \tau_i$, and it can be shown that (see Appendix A.5):

$$\beta \ \cong\ \frac{h_{R_k^-} + h_{R_k^+}\left(\frac{p_1^e}{p_0^e}\right)}{2}\ R_k\ \left(\frac{p_0^e}{p_1^e}\ -\ 1\right) \tag{23}$$

where β is the fraction who bunch at the kink, $p_0 < 1$ is the initial credit-induced low price of donations that rises to $p_1\ =\ 1$ above R_k, $h_{R_k^-}$ is the limit of the density of donations as R approaches R_k from below, and $h_{R_k^+}$ is the limit of the density as R approaches R_k from above. The limits $h_{R_k^-}$ and $h_{R_k^-}$ are from the observed density of donations and the policy parameters R_k and p_t are known. Then if one has an estimate of bunching at the kink β, equation (23) can be solved for the elasticity e. The width of the bunching interval is estimated by $R_k\left(\frac{p_0^e}{p_1^e} - 1\right)$.

We will estimate β using three different methods: *nearest neighbor* (following Saez, 2010), *polynomial* (following Kleven and Waseem, 2013), and our first new approach, that we call *nearest round neighbor*. The methods differ according to the identifying assumption made about how many donors would have located at the kink location in the counterfactual case where the credit is not capped at \$400. The nearest neighbor method assumes that the counterfactual fraction of donors at \$400 would have looked like the average of the two fractions of donors just below and just above the kink: center a bin of bandwidth w at \$400, form one bin below this and one bin above (both of width w), and estimate the regression:

$$f_b = a + \beta d_{b=400} + \epsilon \tag{24}$$

where f_b is the fraction of donors at bin b, $d_{b=400}$ is a dummy indicating the bin centered at

[8]The second new estimator we introduce below makes no functional form assumption on preferences.

\$400 and ϵ is noise. The coefficient $\hat{a}$ estimates the counterfactual fraction at the kink and the coefficient $\hat{\beta}$ estimates bunching at \$400 due to the kink.[9]

When the kink is located at a round number like \$400, the nearest neighbor method cannot avoid capturing in its estimate of bunching at the kink the tendency for people to make donations at round numbers of \$100s, a tendency that has nothing to do with tax policy. This would bias the estimated elasticity away from zero. Applying Kleven and Waseem's (2013) *polynomial* method addresses this round-number bunching by using dummy variables to indicate a donation amount at any multiple of \$100. Accounting for average donations at multiples of \$100 necessarily involves moving away from "near" neighboring bins. Therefore, in addition to the dummy indicator for \$100s, Kleven and Waseem identify the counterfactual fraction of donors at the kink by assuming that the "regular" counterfactual pattern of the distribution can be captured by a third-order polynomial (Chetty, Friedman, Olsen and Pistaferri, 2011 use a similar approach):

$$f_b = a + \beta d_{b=400} + \varphi d_{b\ at\ 100s} + \sum_{j=1}^{3} \omega_j \frac{b^j}{10^{j-1}} + \epsilon \tag{25}$$

where $d_{b\ at\ 100s}$ is a dummy indicating that bin b is a multiple of \$100. Although not shown in (25), the regression also includes round number dummies for donations ending in \$25 and \$50. In (25), as in (24), the counterfactual fraction at the kink is estimated by the prediction of the regression with $d_{b=400}$ set to zero, and $\hat{\beta}$ estimates bunching at the kink. Of course, consistent estimation is based on the regression functional form in (25) being correct in the sense that it adequately captures the shape of the counterfactual fractions across the bins.

We developed the *nearest round neighbor* method to combine the focus on bins that are relatively near the kink, as in Saez (2010), with the recognition that some portion of the fraction at \$400 is there because people tend to make donations at round numbers, as in

[9] Using three bins of equal width w is slightly different than Saez's (2010) use of a bin centered on the kink with a width of $2w$ rather than w. We use equal-width bins so that "bandwidth" is defined the same across the three methods presented in this section. Appendix A.6 shows that using a centered bin twice as wide produces similar but somewhat smaller elasticity estimates.

Kleven and Waseem (2013). The idea is to estimate the counterfactual fraction at \$400 using the fractions at \$300 and \$500, denoted f_{300} and f_{500}. An advantage of the nearest round neighbor method is that a weak identifying assumption—that the counterfactual fractions are monotonically decreasing near the kink—is sufficient to identify lower and upper bounds on the elasticity. For the lower bound: in the extreme case where the counterfactual was a flat line from \$300 to \$400, the counterfactual fraction at \$400 would be simply the observed fraction at \$300 ($f^{cf}_{400} = f_{300}$). The estimate of bunching at the kink is then:

$$\hat{\beta} = f_{400} - f^{cf}_{400}. \tag{26}$$

With decreasing counterfactual fractions, using the fraction at \$300 for the counterfactual would thus provide a lower bound estimate of β and hence a lower bound estimate of the elasticity. Likewise, taking the counterfactual fraction at \$400 to be the observed fraction at \$500 adjusted by p^e_1/p^e_t ($f^{cf}_{400} = f_{500}\,(p^e_1/p^e_t)$), and applying (26), would lead to an upper bound estimate of the elasticity.[10] We also estimate the elasticity using a linearly interpolated estimate of the counterfactual fraction locating at the kink: $f^{cf}_{400} = \frac{1}{2}\,[f_{300}+f_{500}\,(p^e_1/p^e_0)]$.[11]

3D. Kink-based estimation of the uncompensated tax-price elasticity: A second counterfactual

Our second approach uses data from the non-Indiana states, in which donors face neither the credit nor the kink, as "controls". The approach is based on a different counterfactual

[10] The observed density below the kink matches what the counterfactual density would be in the absence of the cap, but the observed density above the kink (with the cap in place) does not. Although in many applications this discrepancy can be ignored (see Kleven, 2016), it is straightforward to adjust the nearest round neighbor method to take it into account. The adjustment is the same as was used in equation (23) to convert the observed density above the kink, $h_{R^+_k}$, to the counterfactual density (see Appendix A.5).

[11] Although monotonically decreasing fractions continuously from \$300 to \$500 is sufficient to identify these lower and upper bounds, all that is necessary is that in the counterfactual, $f_{300} \geq f_{400} \geq f_{500}$, i.e., only that the fraction decreases point-to-point from \$300 to \$400 and that the fraction decreases point-to-point \$400 to \$500. If the counterfactual was increasing, so that the fraction at \$300 was smaller than the fraction at \$500, the bounds would be upper and lower, respectively, and the identifying assumption would be $f_{300} \leq f_{400} \leq f_{500}$ in the counterfactual.

in which the credit is unavailable for all donations, as in the non-Indiana states.

Figure 2 shows that this approach based on the unavailable credit counterfactual estimates an uncompensated elasticity. Under normality, anyone who gives at least R_k in the absence of the credit+kink will stay at an interior solution with giving greater than R_k once the credit+kink is introduced. This means that bunching at the kink will come entirely from individuals who would, were the credit to be eliminated, move from the kink to some lower level of giving below R_k. Consider individual K at the kink; this is the donor whose donations fall by the most after the kink is eliminated. This individual's non-kink equilibrium bundle is represented by the equilibrium S in Figure 2. This individual's indifference curve at K is tangent to the lower edge (with slope $= -1/p_t$) of the kink after the credit+kink is introduced. Because individual K at the kink would relocate to a solution below the kink under the counterfactual, the difference $R_k - R_s$ is an uncompensated price effect.

Two questions arise: How can a single kink capture both bunching from above (Figure 1) and bunching from below (Figure 2), and does bunching from below bias the estimate of the compensated elasticity discussed in Section 4B? The important point to recall is that Figures 1 and 2 represent two entirely different counterfactuals. It is possible that the same individual would give at the kink and would give more if the credit were extended as in Figure 1 (hence she "bunches from above" in Figure 1), but would give less if the credit were eliminated as in Figure 2 (hence she "bunches from below" in Figure 2).[12]

The uncompensated elasticity approach has several benefits. First, if one assumes the distribution of giving in control states can serve as a counterfactual for giving in Indiana (which we discuss momentarily), it is possible to estimate the location of S without any specified utility function at all. Second, because the target of estimation is an uncompensated elasticity that by design includes any income effect, both large-price-change kinks and small-price-change kinks should uncover the uncompensated elasticity well. Third, the approach

[12] In fact, under quasilinear utility it is straightforward to show that the set of $\theta \in [\theta_{min}, \theta_{max}]$ individuals who bunch at the kink is identical in the two counterfactuals. θ_{max} is the individual who would increase her giving the most if the credit were extended, and θ_{min} is the individual who would reduce his giving the most if the credit were eliminated.

can easily accommodate round number bunching. Fourth, as the observed counterfactual distribution of giving is directly observed, this method avoids concerns about the estimation of this distribution raised by Blomquist and Newey (2017) and Bertanha, McCallum, and Seegert (2018). Indeed the estimates under this method can be compared to those using the standard bunching methods below.

Our method for estimating the uncompensated elasticity involves finding the marginal buncher who would reduce his giving the most in a counterfactual where the credit is eliminated, and then using his level of giving to estimate the elasticity. Consider the population of donors in Indiana who make donations in a certain range around the kink, $\Theta = [\underline{R}, \overline{R}]$, where $\underline{R} < R_k < \overline{R}$. Let f be the fraction of donors in the Θ range that are below R_k, so that the percentile value of the *marginal donor* in Indiana just below the kink is $\rho = 100 * f$. This donor is the person whose θ (and giving level) is just below individual θ_{min}, that is, the individual K at the kink in Figure 2 who would reduce his giving the most if the credit were eliminated. Then we take the set of donors residing in the control states who give amounts in the Θ range, find the ρ–percentile donor, and use that donor's giving amount $R(\rho)$ as an estimate of what the marginal donor in Indiana would give in the counterfactual where the credit is eliminated.[13] The arc (uncompensated) elasticity of giving is then:

$$\hat{e}_t^{\text{un}} = \frac{[R_k - R(\rho)]/[(R_k + R(\rho))/2]}{[p_1 - p_0]/[(p_1 + p_0)/2]} \tag{27}$$

The greater the bunching at the kink in Indiana, the lower the percentile value ρ of the Indiana donor just below the kink, consequently the lower will be the amount $R(\rho)$ from the control states, and the larger will be $\hat{e}_t^{\text{un}}$.[14]

[13]As a concrete example, consider gifts from \$201 to \$1000, so that $\Theta = [201, 1000]$. For gifts in Indiana, a gift of \$399 would be the $\rho = 49.4^{\text{th}}$ percentile of gifts in this range. Outside of Indiana, the $\rho = 49.4$-percentile level of giving in this range is $R(\rho) = \$335$.

[14]We investigated the presence of credits in other states for donations to the university located in Indiana, and cannot identify any other credit in these states that would bias the results. If there were a set of control states that offered an uncapped credit for donations, we could use them and the percentile-based approach to estimate the marginal individual bunching from *above* the kink and thereby estimate the compensated elasticity; this would be an alternative to the kink methods described in Section 3C. Without such a set of control states, this percentile method will estimate the uncompensated elasticity below the kink.

The baseline estimate pools donors in the Θ range from all control states to find $R(\rho)$. The identifying assumption is that donors in other states can be used to study giving behavior in Indiana in the absence of a state tax credit. The data come from a school in Indiana, so that donors in Indiana not only face a credit, but include alumni who stay in-state after graduation; it is possible that alumni residing in the other states differ in unobserved ways. Accordingly, we do several checks intended to diagnose problems with the identifying assumption.

First, we solicited qualitative information from university administrators who work closely with alumni and donors. The administrators report that donors in Indiana are, in terms of age, income, and "school spirit," similar to donors in other states. This qualitative indicator of similarity, like any check of an identifying assumption, is a necessary though not sufficient condition.

Second, to the extent that there is heterogeneity across states in how similar their donors are to Indiana donors, we can exploit variation in the $R_j(\rho)$ amounts from the $j = 1, . . ., N_{states}$ separate states to construct "heterogeneity lower and upper bounds". To do this, find the ρ–percentile donor in each state separately, and form a set of those $\{R_j(\rho); j = 1, 2, ..., N_{states},\ j \neq \text{Indiana}\}$. The smallest $R_j(\rho)$ amount from this set, when used in (27), will produce the largest $\hat{e}_t^{\text{un}}$ from among the control states. At the other extreme, the largest $R_j(\rho)$ amount from this set will produce the smallest $\hat{e}_t^{\text{un}}$. The smallest and largest $\hat{e}_t^{\text{un}}$s estimated in this way are the lower and upper bounds constructed from the full range of heterogeneity across the states. Using the smallest-to-largest $\hat{e}_t^{\text{un}}$ interval to bracket the uncompensated elasticity involves a much weaker identifying assumption: that at least one state in that interval can serve as a control state for Indiana.

Third, if unobserved heterogeneity causes differences in the giving of donors in Indiana compared to the giving of donors in other states—for example, if in-state alumni were especially fervent supporters of the university and especially generous donors—then it would be likely that we would find a nonzero spurious elasticity at some other location above the kink, say at \$500—or even at \$401. We check for this possibility by redoing the estimation using a series of "placebo kinks" above the true kink.

However, unobserved heterogeneity would not be the only possible interpretation of a sizable "elasticity" at a placebo kink above the true kink; an alternative interpretation would be that the tax credit produces a large income effect. To understand why, return to the first counterfactual depicted in Figure 1 and note that a portion of the budget constraint (the part below the kink) is exactly the same both before and after the kink is introduced. But that is not true in the second counterfactual: in Figure 2 the donors in Indiana are always on a different budget line than those in the control states. In this figure, for a person in Indiana giving slightly above the kink, say \$450, the tax credit works as a pure income effect. For this Indiana donor, the price of donating one extra dollar is the same as it would be in any other state—but the Indiana donor has \$200 more income than she would in the control states because she qualifies for the \$200 Indiana credit. Now assume for the moment that there are no income effects at all; then the Indiana donor would be unresponsive to the \$200 income shock created by the credit, and would give the same \$450 even if the credit were eliminated. This argument holds not just for \$450 but for any value of giving above \$400: in the case of no income effects, the distribution of donors giving more than \$400 in Indiana would match the distribution of donors giving more than \$400 in the control states (it is straightforward to verify this in the quasilinear model; see Appendix A.7). Relaxing the assumption of no income effects: if income effects are positive but small, the distributions of giving in Indiana and the control states should be similar and a placebo kink above the true kink should return a near-zero estimate.

We interpret placebo kink checks as primarily informative about heterogeneity because our prior expectation is that income effects in this giving environment are likely small, because the \$200 income shock is a very small percentage change in donors' incomes. Alternatively, if one expects large income effects, a sizable "elasticity" at a placebo kink would be indicative of either unobserved heterogeneity or a sizable income effect or both. In any event, elasticities near zero at placebo kinks above the true kink, combined with an elasticity estimate at the true kink similar in magnitude to the compensated elasticity estimates from Section 3C, would suggest the $\hat{e}_t^{\text{un}}$ results are being driven by bunching at the tax kink and not by heterogeneity in unobservables or by large income effects.[15]

Finally, the identifying assumption for the second counterfactual is qualitatively different from the identifying assumptions used in the variety of Section 3C kink methods which do not use control state information in any way. Summarizing then, although the identification assumption in (27) should be kept in mind while thinking about the $\hat{e}_t^{\text{un}}$ estimates, we can bound e_t^{un} under a weak identifying assumption, carry out strong tests of robustness using placebo kinks, and compare $\hat{e}_t^{\text{un}}$ to the Section 3C estimates of the compensated elasticity that rely on qualitatively different identifying assumptions.

4. Parallel e_m and e_t estimates

4A. Estimates of e_m

Figure 3 presents simple visual evidence of the response to the matching grant. For the 1960 class we aggregate the amounts received by the university in each month, take the log, and smooth the data for the figure by averaging the logged amounts received over six month periods from 2007 through 2012. Because December 2008 is the first month of the match, it is averaged in with the first half of 2009. We do the same thing for the nearby control classes 1954 to 1959 and 1961 to 1965, averaging the log of aggregate monthly amounts received for these classes over each six month period. Figure 3 plots the difference: that is, 1960-class giving minus giving from other nearby classes.[16]

[15] Below the kink, interpretation of placebo estimates is more complicated, even if income effects are zero. It can be shown that for placebo kinks at a distance below the true kink (e.g., placebo kinks at \$200 or \$250) the "elasticity" estimate should be close to zero, but as the placebo kink location approaches the true kink from below (e.g., at \$390) the placebo "elasticity" approaches the true elasticity (see Appendix A.7); this pattern is confirmed in the data. We focus on placebo locations above the kink because placebos above the kink provide a sharper robustness test of unobserved heterogeneity.

[16] To facilitate comparison with the tax-rebate elasticities to be estimated below, we focus on joint donations in nominal dollars. Because the treatment varies by graduation class, the sample is restricted to alumni.

There is a clear spike in the amount received from the 1960 class relative to the nearby classes during the period of the match, especially in the first half of 2010. After the match switches off, the amounts received from the 1960 class once again resemble unmatched donations from the nearby classes.

In Table 2 we use this response to estimate a match-price elasticity. The dependent variable is the log amount received by the university from each separate checkbook donation. The first row estimates are the matching-treatment dummies, δ, from Section 3B equation (21). The second row converts the δ into an elasticity as described in equation (22). Each regression includes class, year, and month dummies. Moving left to right across the columns adds state dummies and different controls for trends. The standard errors are clustered by graduating class. The baseline $\hat{\delta}$ in column 1 suggests about an 84 percent increase in the size of the amount received from any checkbook donation made when the match is available, which is similar to the overall implied effect in Figure 3. The implied elasticity is -1.265 (s.e. $=$ .074). Column 2 adds month-by-year dummies, column 3 adds state-of-residence dummies, and column 4 adds state-by-year dummies; in each specification the estimates are similar to baseline. Column 5 includes class-specific year trends. Unsurprisingly, given the identifying source of variation in the data, the estimates are quite similar with these controls.

In Table 3 the dependent variable is the amount received aggregated into class $\times$ state $\times$ month $\times$ year cells (then logged). Because the difference between states captures the difference between the number of donations coming from each state, as well as the difference between the amount received per donation in each state, the estimates combine both extensive and intensive margin changes in response to the match. The baseline elasticity estimate is -1.208 (.095). The results are similar across the specifications.[17] The last column investigates how the total amount received of the 1960 class changes after the match switches

Similar estimates are produced if these restrictions are relaxed.

[17]The similarity of the results in Tables 2 and 3 implies that the match did not increase the number of donations. Appendix A.8 Table AT1 confirms this by estimating models in which the dependent variable is the number of donations. Hence, extensive margin results indicate that the match was ineffective at encouraging "cold donors" (alumni who after approximately 50 years were not giving to their alma mater) to start giving.

off. The model is the month-by-year specification from column 2, plus a new dummy variable that equals one in the 12-month period following the match. The coefficient on the new dummy is essentially zero, and precisely estimated: after the match switched off, the donation behavior of the 1960 class cannot be distinguished from that of other classes. As in Figure 3, there does not appear to be a long-term increase, or decline, in donations after the match ends.

4B. Estimates of e_t: Compensated

Figure 4 provides graphical evidence about the nature of bunching at the kink. The figure presents a histogram of joint dontions between \$200 and \$700, in bins of \$10, for donors from Indiana (grey bars) and elsewhere (clear bars). Even in this somewhat narrow range of donation amounts there are over 7,000 donors in Indiana alone. While both groups see much higher densities of giving at \$400 than \$10 above or below that amount, the figure shows evidence of particularly large bunching for those in Indiana compared to other states. The pattern of declining densities at multiples of \$100 is broken at \$400 in Indiana, but not so in other states. Hence, a simple visual inspection suggests that the tax incentive at least to some extent "matters" in Indiana. In this section we use the excess bunching in Indiana to estimate the compensated tax rebate-price elasticity.

Table 4 presents results using the three methods described in Section 3C. Row 1 begins with the nearest round neighbor method from equation (26). The first column presents the lower bound estimate where the counterfactual is based on the fraction giving at \$300. The last column shows the upper bound estimate using \$500 as the counterfactual, and the middle column is the linearly-interpolated counterfactual. The lower bound estimate is $-.121$. The upper bound estimate is $-.293$. Both estimates have small standard errors (.021, .033). The -.121 to $-.293$ range is fairly narrow; that is, the lower and upper bounds are informative. The point estimate of the elasticity in the middle column is $-.197$ (.024); the 95% confidence interval is $-.150$ to $-.243$.

Row 2 presents the nearest neighbor estimate developed by Saez when a bandwidth of $25 is used: -.465 (.036). It is important to understand why this estimate is larger (more negative) than the nearest round neighbor estimates in row 1. First, note that with a bandwidth of $25, the estimate in row 2 is comparable to a nearest round neighbor estimate that uses mass points at $375 and $425 instead of $300 and $500. Second, the fractions of donors at $375 and $425 are very small and not typical of donation amounts that are multiples of $100, as can be seen upon examination of Figure 3. Therefore the row 2 estimate confounds bunching at the kink because of the tax policy with the greater tendency to donate at $400 because it is a multiple of $100. When the nearest neighbor bandwidth is expanded to $50 (row 3), so that the neighbor below the kink includes the mass point at the round number $350 (as well as the mass point at $375) and the neighbor above the kink includes the mass point at round number $450 (as well as the mass point at $425), the estimate falls to -.290 (.019). Obviously, though, all these estimates are far below the e_m estimates in Tables 3 and 4. The polynomial method is presented in row 4. The elasticity estimate is -.259 (.022).[18]

Rows 5-8 return to the nearest round neighbor method and examine its sensitivity to various estimation choices. Row 5 doubles the bandwidth used to estimate the counterfactual density below and above the kink; the change in estimates is negligible compared to the baseline in row 1. Row 6 doubles to $50 the width of the bins into which we put the donation amounts; the resulting estimates are smaller magnitude, the lower bound not being significantly different from zero. Row 7 uses the mass point at $250 (in place of the mass point at $300) in the estimation of counterfactual fraction who choose the kink because the

[18]The polynomial estimate was produced using donation amounts from $200 to $999, a range we selected out of concern that large mass points at amounts outside that range (at $25, $50, $100 and $1,000) may distort the polynomial from accurately capturing the counterfactual pattern of the distribution around $400. Accordingly, we examined the sensitivity of this method to the choice of range: expanding the range to the left to include $100, $50 and $25, and expanding the range to the right to include $1,000 and $1,500, as well as doing sensitivity analyses of other estimation choices: doubling the bandwidth, doubling the bin width, using different polynomials (linear through fifth-order), using no polynomial (i.e., using just the round number dummies), using only the bins at multiples of $100, and "dummying out" the football lottery-influenced mass point at $200 so that it does not contribute to forming the counterfactual fraction at the kink. These sensitivity tests produced a range of estimates that in no case changed the substantive findings of the table: the smallest magnitude was $-.017$ (s.e. = .018; using a linear polynomial) and the largest $-.369$ (s.e. = .026; using a fourth-order polynomial).

kink is at a round number: the linearly interpolated estimate is smaller (−.136), the lower bound is essentially zero, and the upper bound is, of course, unaffected. Row 8 uses the mass point at $600 (in place of the mass point at $500). The resulting linearly interpolated estimate (−.231) is not much different than baseline, but the upper bound estimate (−.477) is larger. This upper bound estimate based on the fraction at $600 is a more conservative upper bound because the upper bound based on the fraction at $500 (row 1) may capture a tendency to give in multiples of $500, over and above the tendency to give in multiples of $100. In any event, the main conclusion from Table 3 remains: regardless of which estimator we use, the elasticity estimates reflect evidence of clear bunching but are uniformly inelastic and smaller than .50 in magnitude.

Table 5 subjects the nearest round neighbor method to a series of placebo tests. Each row provides estimates of "elasticities" at the placebo kink listed in column 1. For example, row 1 shows the elasticity estimates from a placebo kink at $300, using mass points at $250 and $500 as the nearest round neighbors. The lower bound and linearly interpolated estimates are nonsensically positive, and the upper bound is a very small −.096. Row 2 examines a placebo kink at $500: the lower bound estimate is positive, the linearly interpolated estimate is −.110, and the upper bound is −.240. These larger negative placebo results are consistent with the point raised in the previous paragraph that the fraction at $500 may capture a tendency to give in multiples of $500 over and above the tendency to give in multiples of $100. The six remaining linearly interpolated estimates in rows 3-8 include two that are negative but small (−.078 and −.084) and four that are positive. The remaining upper bound estimates include two that are positive and four that range from −.059 to −.166, magnitudes much smaller than Table 2's −.293 baseline upper bound and −.477 more conservative upper bound. In short, these tests indicate that the estimates based on the mass point at the $400 kink are picking up more than just a placebo.

In summary, the estimates suggest a compensated tax-price elasticity between −.121 and −.293, with a more conservative upper bound estimate of about −.477. Estimates from nearest neighbor (−.290) and polynomial (−.259) methods are smaller in magnitude than the conservative upper bound. The standard errors on these estimates are fairly small.

4C. Estimates of e_t: Uncompensated

Table 6 provides uncompensated elasticity estimates as discussed in Section 3D and equation (27). The table focuses on a $\Theta = [\$201, \$1{,}000]$ range around the kink, but the results are similar using alternate ranges (see Appendix A.8 Table AT3). The baseline elasticity estimate is −.265 (.042); the 95% confidence interval is −.183 to −.347. Rows 2 and 3 present the heterogeneity lower and upper bounds: from zero to −.429. The overall similarity of these results with Section 4B's compensated elasticity estimates suggests that income effects do not create dramatic differences between the uncompensated and compensated elasticities.

Rows 4 (a)-(d) test the sensitivity of the control state identifying assumption by looking for "elasticities" at placebo kinks above the true kink. If the distribution of donations in Indiana differs from other states in a way that biases the estimates in rows (1)-(3), then we would also expect to see this bias leading to spurious estimates not only at the true $400 kink, but at other amounts above this kink as well. In row 4(a) going just one dollar above the real kink reveals a strikingly different estimate. The estimate in this case is a wrong-signed .069 and insignificantly different from zero. The estimates remain close to zero at placebo locations farther above the kink. These results thus show that the uncompensated elasticity estimates are local to the true kink, and that when looking at other donation levels close to but not precisely at the kink, the distribution of donations is similar in Indiana compared to the control states. This does not support a heterogeneity story where donors in Indiana are simply more generous at all levels of giving, but does indicate that the estimates are driven by bunching precisely at the kink.

To summarize, the estimates indicate an uncompensated tax-price elasticity that is inelastic, small and precisely estimated. The estimates are close in magnitude to the compensated elasticity estimates from Section 4B. Hence, kink-based approaches based on two different counterfactuals produce tax-price elasticities that are similar to each other, but are both much smaller than the match-price elasticity. In the next section we discuss implications of these estimates.

5. Discussion

The empirical work in Section 4 indicates a large, elastic response of the amount received by the university to a match, but a small, inelastic response to a rebate. Using the baseline estimates of $e_m = -1.208$ from Table 3 and $e_t = -.197$ from Table 4, the estimated difference is $-1.208 - (-.197) = -1.01$ (bootstrapped s.e. = .0908). The difference between e_m and e_t is large in magnitude and highly significant. We can therefore unambiguously reject the pure impact model's prediction that the match and rebate elasticities are equivalent. We can also reject the pure warm-glow model; that model predicts the amount received in response to a match is mechanically unit elastic because donors do not change their checkbook amount at all. However, the estimates from Table 3 (e.g., the baseline $e_m = -1.208, s.e. = .095$) reject $e_m = -1$. In other words, despite this being a setting with many donors and a large amount being given to the public good—a setting which might be expected to produce, on the margin, a response indistinguishable from pure warm-glow—there is evidence that on the margin donors are still influenced by the match. This evidence, combined with the non-equivalence evidence that implies donors also care about about their checkbook giving, is consistent with donors getting utility from both the amount the university receives because of their checkbook donation and the checkbook amount itself—the impure impact model developed in Section 2.

Vesterlund (2016) notes that only one paper in the prior literature, by Davis (2006), manages to undertake a treatment that leads to similar match and rebate effects on donations. That treatment is based on the idea that donors may "isolate on the amount presented to be divided" between oneself and the public good. By changing the framing of the decisions, donors' focus can be shifted from the checkbook amount to the impact amount, and rebates and matches can have similar effects.[19] Isolation thus relies on the impure impact models' two key components g_i and R_i. However, while donors may sometimes be induced to isolate their attention on just g_i or R_i, in the empirical setting we investigate both effects are operative.[20]

Moreover, the large but different from -1 match elasticities estimated in Section 4A are in line with results from previous match studies. For instance, Karlan and List's (2007) field experiment estimated a checkbook elasticity of about $-.3$, implying an amount received elasticity of about -1.3. Across a series of papers, including the first to produce experimental evidence of non-equivalence, Eckel and Grossman have found match elasticites ranging from -1.05 to -2.6 (2003, 2006a, 2008, 2017). Scharf and Smith's (2015) analysis of taxpayers' intended responses to an increase in the match indicated an elasticity of -1.45.[21] Other studies report similarly elastic responses (e.g., Lukas, Grossman, and Eckel 2010; Kesternich, Löschel, and Römer 2016).[22]

[19] A related explanation is offered by Scharf and Smith (2015). They further point out that it is reasonable to think that short-run responses at work in experiments could be different from long-run responses to matches and rebates, as over time donors adjust to incentives. This is further motivation for the empirical results, because the match we investigate caused a 50 percent reduction in p_m and donors had 19 months to become accustomed to it, and the tax credit caused a 50 percent p_t reduction and has been in place for five decades.

[20] Additionally, work such as Lukas, Grossman, and Eckel (2010) notes that an implication of isolation theory is that donors should be unresponsive to all match sizes. Although there is evidence that donors do appear to eventually become unresponsive as matches become large (Karlan and List, 2007), there also is evidence that variation in small matches can matter (e.g., see Huck, Rasul, and Shephard, 2015). As discussed below, the impure impact model is compatible with both findings.

[21] Huck and Rasul (2011) estimate a positive checkbook elasticity (e.g., for $p_m = .50$, the elasticity is $+.211$), and argue that the difference compared to prior experiments' negative checkbook elasticities is because the Huck and Rasul design nets out a leadership gift effect: simply knowing that there is a leadership gift, even if it is not a matching grant, encourages people to donate. The design in prior experiments produces elasticities that would implicitly include any leadership gift effect. When Huck and Rasul replicate this prior design so that their elasticity estimate also includes the standard leadership gift effect, the estimates are comparable with the checkbook elasticities from prior experiments (see the discussion of Table 3 in their paper). Like prior match studies, the match-price elasticity we estimate implicitly includes any leadership effect, so the present estimates are compatible with the prior studies. And, of course, the model in Section 2 allows individuals to have different preferences over checkbook, matching, and leadership gifts.

[22] Situating this result in the non-experimental tax-price literature is less straightforward because that literature has produced a wide range of findings, so that *any* number produced will be close to some previous papers but not others. Papers reporting inelastic estimates include Barrett, McGuirk, and Steinberg (1997), Kingma (1989), Bradley, Holden, and McClelland (2005), Randolph (1995), and Almunia, Lockwood, and Scharf (2018). Perhaps most notably, Fack and Landais (2010) estimate a tax-price elasticity reasonably

The theory from Section 2 implies that estimates of the rebate elasticity, but not the match elasticity, can be used to estimate the *compensated* price elasticity of giving. The empirical results from Section 4 lead to two such estimates. First, the estimates in Section 4B directly produce compensated elasticities, and as discussed above, the baseline estimates among these is $-.197$. Second, Section 4C's baseline uncompensated estimate ($-.265$ from Table 6) can be combined with the Slutsky decomposition from equation (12) and an estimate of the income effect to produce a compensated elasticity. List (2011) shows that the proportion of income spent on charitable giving appears reasonably steady across income levels and time, at about three percent. So, setting the income effect $q_1 = .03$ and prices to unity leads to an estimate of the compensated elasticity of $-.265 + .03 = -.235$. Thus, two different methods of estimating compensated effects—one from direct estimation and the other from a Slutsky decomposition making no additional assumptions about utility functional form—produce similar results. To our knowledge, these are the first estimates of compensated price elasticities of giving.

We conclude this section with the limitations that should be kept in mind when thinking about the results. The real-world parallel estimates of match and tax rebate elasticities we present are for giving to a university. Although the estimates are qualitatively similar to those from investigations of other types of charitable giving—both from experiments producing parallel estimates, and from studies producing match-price effects alone—further work investigating non-equivalence in a variety of settings is warranted. Next, the present theoretical result that the presence of stronger warm-glow pushes the amount received elasticity $e_m \to -1$ (hence the checkbook elasticity $\to 0$), combined with Ribar and Wilhelm's (2002) result that warm-glow can become stronger on the *margin* at larger levels of the charity's exogenous output, suggest that impure impact may provide insight about a puzzle first noted

close to the present estimates in an empirical setting where, like ours, the price variation occurs while income is held constant (otherwise their setting is different: they investigate total charitable giving in France, and a credit capped at a much higher level, 20 percent of taxable income). The results we report are generally close to most prior tax-price work that uses either credits or experimental methods to avoid confounding income effects with tax-price effects.

by Karlan and List (2007): that matches become less effective at increasing the checkbook amount as the match rate grows. Intuitively, because matches increase R_i, holding g_i constant, then as the match rate grows the role of warm-glow may become more prevalent among donors, so that checkbook giving becomes less responsive on the margin to changes in the match. However, this intuition could depend on degree of substitutability between the amount received and the checkbook amount. An exploration of this possibility is left for future work. Next, the evidence of strong warm-glow we found suggests the difference between rebates and matches matters for donor welfare, at least in the setting we investigate, but with the data at hand we cannot directly measure the welfare difference. Eckel and Grossman (2006b) report evidence that when presented with the choice more people prefer rebates over matches, which suggests further investigation of impure impact's normative implication.

6. Conclusions

This paper introduces a theory of impure impact giving that combines the extant pure impact model with warm-glow. The theory has several implications. It predicts non-equivalent responses to price-equivalent match and rebate incentives. The reason is that the match-price elasticity combines the effects of a conventional price elasticity (which is the rebate elasticity) with warm-glow. Warm-glow has two opposing effects on the match elasticity: warm-glow mitigates the substitution effect but enhances the "income" effect that arises from the charity's exogenous output. Because of its two effects, warm-glow can make a match more effective, compared to a rebate, in increasing the amount received by the charity, but it is possible that warm-glow counter-intuitively makes a match less effective. Impure impact implies that a match elasticity, estimated in isolation from all other information, cannot be used to identify the conventional price elasticity of impact separately from the strength of warm-glow. It follows that, general speaking, one should not expect an estimate of a match elasticity—either the amount received or the checkbook elasticity—to compare to the price-equivalent rebate elasticity; match and rebate elasticities are fundamentally different entities. Finally, impure impact has a normative implication: donor welfare is higher under a rebate than a price-equivalent match.

Non-equivalence between match and rebate elasticities has previously been found in experiments. Our empirical investigation of giving to a university provides the first evidence of the external validity of non-equivalence based on parallel estimates of match and rebate elasticities generated from an incentivized match and a real-world tax rebate. The estimates indicate an elastic response of the charity's amount received to the match (like much of the previous match literature), but a small, inelastic response to the rebate. The results reject equivalence, and hence the extent pure impact model. The results also reject pure warm-glow, indicating that donors do get some utility from the university's larger amount received evoked by the match. Consequently, the results are consistent with impure impact.

These results suggest that investigating the external validity of non-equivalence in additional settings, determining the practical significance of the difference in donor welfare between rebates and matches, and estimating compensating elasticities to more fully describe an optimal tax system for giving (e.g., as in Saez, 2004) are important areas for future research.

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Figure 1. Extended credit counterfactual: Compensated price effect.

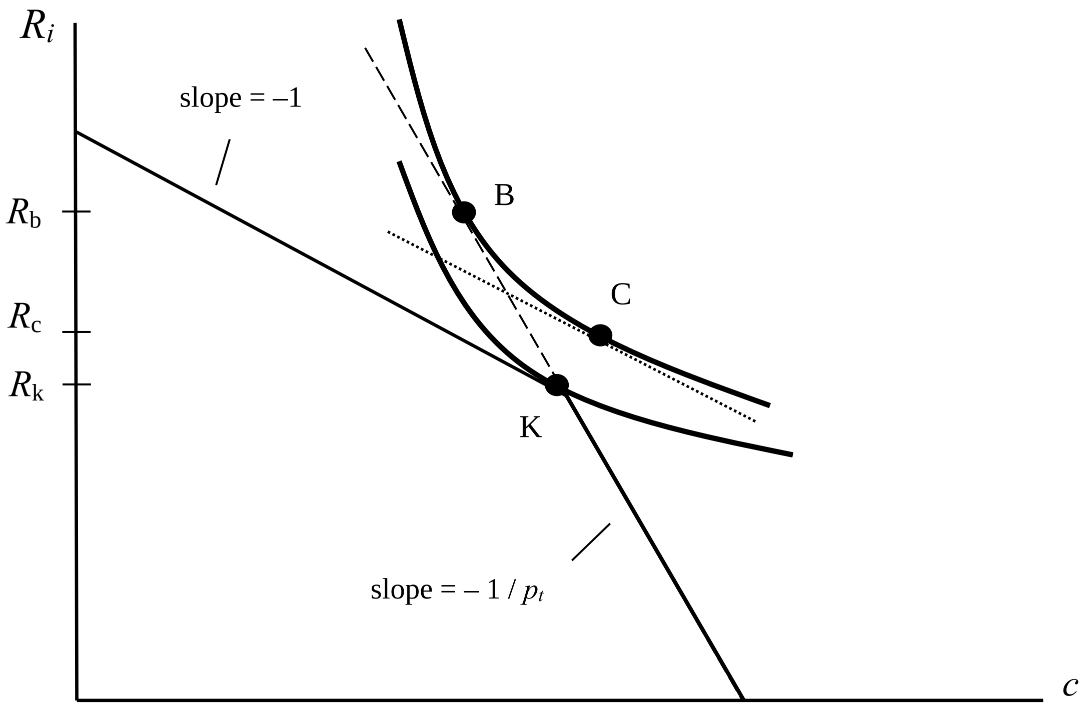


Figure 2. Unavailable credit counterfactual: Uncompensated price effect.

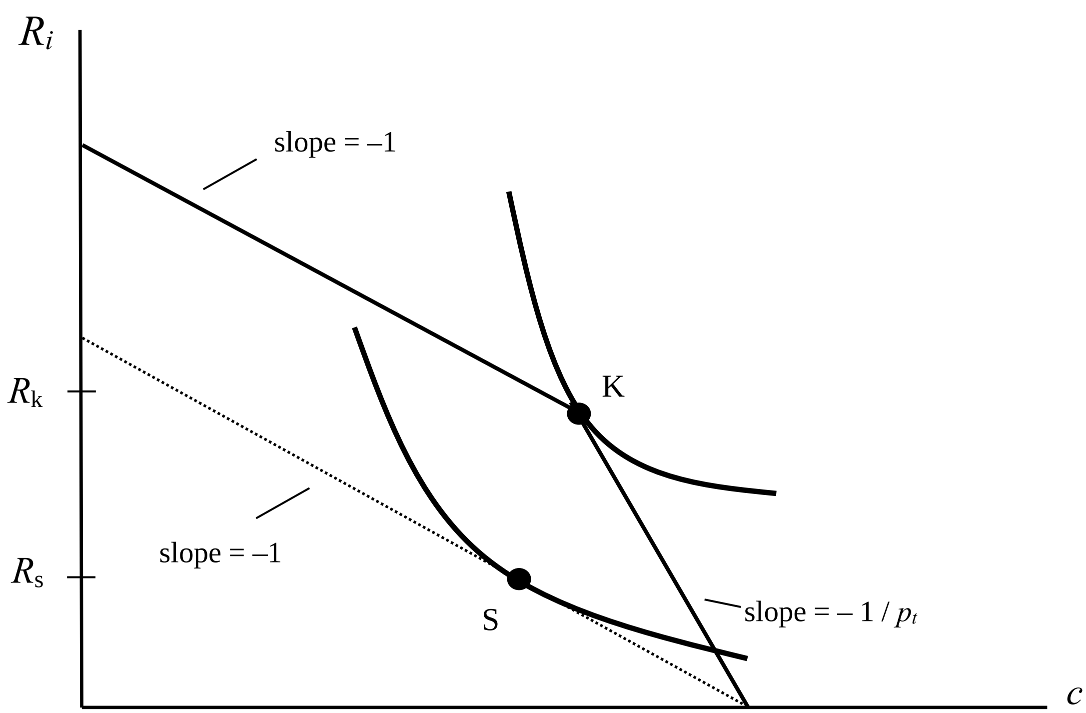
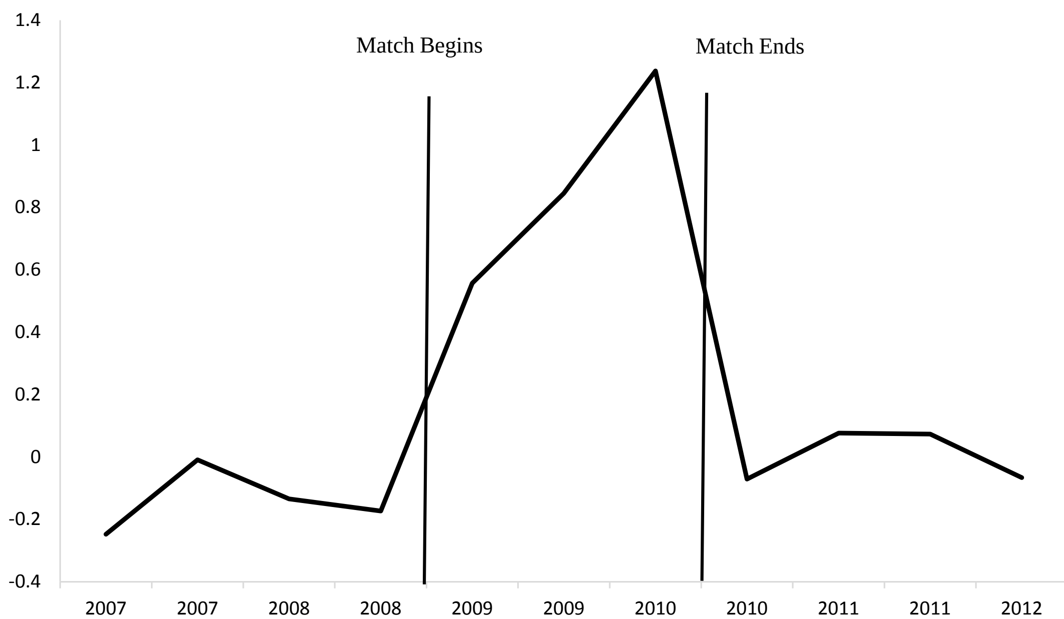
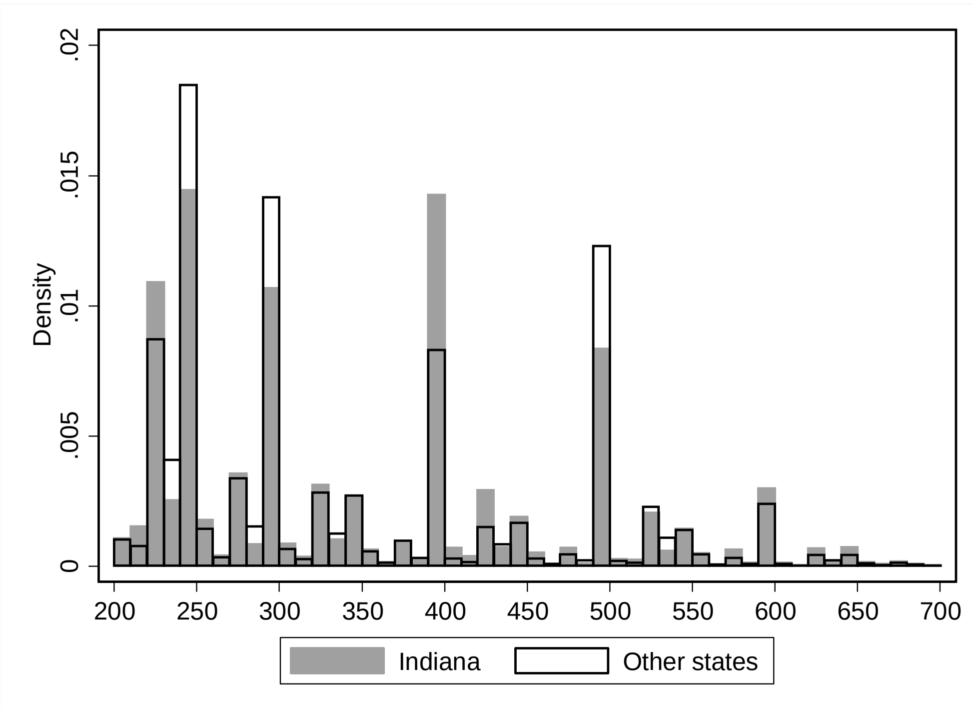


Figure 3. Log amounts received: The 1960 class minus nearby classes.



Notes: The figure plots the difference between total amount of donations received from the 1960 class and the nearby classes 1954–1959, 1961–1965. For the 1960 class we aggregate the donations from all class members in each month, take the log, and then average the logged donations over six month periods from 2007 through 2012. Because December 2008 is the first month of the match, it is averaged in with the first half of 2009. We do the same thing for the nearby control classes, and take the difference.

Figure 4. Joint donations from Indiana and other states: A tax kink exists at $400 in Indiana.



Note: The figure shows a histogram of joint donations greater than $200 and less than $700 from Indiana (grey bars) and other states (clear bars) between 2004 and 2015 in bins of $10. There are 75,068 donations in the picture, of which 7,128 are from Indiana.

Table 1. Summary statistics: Checkbook donations.

	All donors[a]
1. Number of donations	471,861
2. Average donation	2,067 (65,344)
3. Number of donations from the 1960 Class	8,321
4. Average donation from the 1960 Class	4,360 (135,321)
5. Number of annual donations†	373,994
6. Average annual donation†	3,221 (87,995)
7. Number of annual donations between $200 and $1,000†	79,122
8. Average annual donation between $200 and $1,000†	374 (149)

Notes: The statistics describe joint checkbook donations to the university between 2004 and the first five months of 2015, and are calculated using donations from people in all states. Standard deviations are in parentheses.

Among the donations described in the table are 41,129 annual joint donations from people living in Indiana. Of these donations 7,585 are between $200 and $1,000, and in this range the average donation is $383 (sd = $155).

[a] Donors in all the states. There are more than 50 “states” because there are donations made from several territories and by people in the military: American Samoa, the Federated States of Micronesia, Guam, the Mariana Islands, the Armed Forces Americas, Armed Forces Europe, Armed Forces Pacific, Puerto Rico, Palau, and the Virgin Islands.

† Donations in the last 4 rows are donations aggregated to the annual level (that is, all donations made by a person during the year are combined). In the first 4 rows non-aggregated donations are reported.

Table 2. Match-price elasticity: Difference-in-differences estimates using donation-level observations (intensive margin).

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends
Matching treatment for the 1960 class (δ)	.843	.841	.842	.840	.845
	(.049)	(.048)	(.048)	(.047)	(.041)
Implied elasticity (e_m)	−1.265	−1.261	−1.264	−1.260	−1.267
	(.074)	(.072)	(.072)	(.071)	(.062)
Matching treatment placebo for the other classes[a]	Yes	Yes	Yes	Yes	Yes
25^{th}, 50^{th}, 75^{th} graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No
Month by year dummies	No	Yes	Yes	Yes	No
State by year dummies	No	No	No	Yes	No
Class trends	No	No	No	No	Yes

Notes: The dependent variable is the log of the amount received by the university (checkbook donation plus the match). The sample are N = 471,861 separate alumni donations. The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (22) with p_m = 1/2 and p_1 = 1; delta method standard errors are in parentheses. Using real dollars, including non-joint donations, or including non-alumni donations produces similar results.

[a] A dummy that equals one for the other classes during the 19 month period around their 50^{th} anniversaries.

Table 3. Match-price elasticity: Difference-in-differences estimates using aggregated observations (extensive and intensive margins).

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends	Post-match control
Matching treatment for the 1960 class (δ)	.805	.818	.820	.819	.812	.818
	(.063)	(.062)	(.064)	(.064)	(.041)	(.063)
Implied elasticity (e_m)	−1.208	−1.227	−1.23	−1.229	−1.217	−1.227
	(.095)	(.093)	(.096)	(.096)	(.062)	(.095)
Dummy for the 12-month period after the match for the 1960 class	-	-	-	-	-	.0004
						(.025)
Implied post-match elasticity	-	-	-	-	-	.0006
						(.038)
Matching treatment placebo for the other classes[a]	Yes	Yes	Yes	Yes	Yes	Yes
25th, 50th, 75th graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No	No
Month by year dummies	No	Yes	Yes	Yes	No	Yes
State by year dummies	No	No	No	Yes	No	No
Class trends	No	No	No	No	Yes	No

Notes: The dependent variable is the amount received by the university aggregated by each class in a state, month, and year (then logged). There are 156,589 class x state x month x year cells. The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (22) with $p_m = 1/2$ and $p_1 = 1$; delta method standard errors are in parentheses. Using real dollars, including non-joint donations, or including non-alumni donations produces similar results.

[a] A dummy that equals one for the other classes during the 19 month period around their 50th anniversaries.

Table 4. Rebate-price elasticity: Compensated elasticity estimated using a tax credit.

Row	Method	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
1	Nearest round neighbor[a]	−.121	−.197	−.293
		(.021)	(.024)	(.033)
2	Nearest neighbor[b]		−.465	
			(.036)	
3	Nearest neighbor[c]		−.290	
			(.019)	
4	Polynomial[d]		−.259	
			(.022)	
	Nearest round neighbor[e]			
5	Use two bins below and two bins above the kink to estimate the counterfactual density (i.e., double the bandwidth)	−.111	−.177	−.257
		(.022)	(.021)	(.023)
6	Double the bin width to $50	.006	−.098	−.224
		(.020)	(.019)	(.021)
7	Mass point at $250 (instead of $300)	.045	−.136	−.293
		(.021)	(.023)	(.033)
8	Mass point at $600 (instead of $500)	−.121	−.231	−.477
		(.021)	(.025)	(.035)

Notes: Bootstrapped standard errors are in parentheses. The estimates use equation (23) where $p_t = 1/2$ and $p_1 = 1$.

[a] Mass points at $300 and $500 used to identify counterfactual bunching at the kink. The lower bound is based on the mass point at $300, the upper bound is based on the mass point at $500, and the point estimate is based on linear interpolation between the $300 and $500 mass points. Donation amounts are placed in bins of width $25, centered at round numbers (e.g., $375, $400, $425, etc.). One bin below and one bin above the kink are used to estimate the counterfactual density.

[b] Bandwidth = $25. Therefore, the kink includes donation amounts in the interval ($387.50, $412.50). One band below the kink ($362.50, $387.50] and one band above the kink ($412.50, $437.50) are used to identify counterfactual bunching at the kink, and to estimate the counterfactual density. The method is due to Saez (2010).

[c] Bandwidth = $50. Therefore, the kink includes donation amounts in the interval ($375, $425). One band below the kink ($325, $375] and one band above the kink ($425, $475) are used to identify counterfactual bunching at the kink, and to estimate the counterfactual density.

[d] Third-order polynomial in the donation amounts from $200 to $999 and three dummy variables at round numbers (at 25s, 50s and 100s) used to identify counterfactual bunching at the kink. Donation amounts are placed in bins of width $25. One bin below and above the kink are used to estimate the counterfactual density. The method is due to Kleven and Waseem (2013).

[e] Same estimator as in Note [a] but with the indicated modifications to the estimation parameters.

Table 5. Nearest round number estimator applied to placebo kinks.

Row	Placebo kink at	Mass points used for identification	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
1	$300	$250 and $500	.125 (.015)	.087 (.014)	−.096 (.019)
2	$500	$300 and $600	.116 (.025)	−.110 (.022)	−.240 (.027)
3	$600	$500 and $700	.383 (.046)	.119 (.026)	−.166 (.024)
4	$700	$600 and $800	.334 (.068)	.228 (.055)	.098 (.043)
5	$800	$700 and $900	−.057 (.020)	−.084 (.020)	−.112 (.023)
6	$900	$800 and $1,100[a]	.263 (.068)	.292 (.072)	.379 (.140)
7	$1,000	$500 and $1,500	.221 (.028)	.063 (.014)	−.059 (.015)
8	$1,500	$1,000 and $2,000	−.056 (.006)	−.078 (.006)	−.100 (.007)

Notes: Donation amounts are placed in bins of width $25. One bin below and one bin above the kink used to estimate the counterfactual density. Bootstrapped standard errors are in parentheses. The estimates use equation (23) where $p_t = 1/2$ and $p_1 = 1$.

[a] The right mass point is set at $1,100 to avoid excessive round-number bunching at $1,000.

Table 6. Rebate-price elasticity: Uncompensated elasticity estimated using a tax credit.

Row	Method		Estimate
1	Baseline[a]		−.265
			(.042)
2	Heterogeneity lower bound[b]		0
			(.082)
3	Heterogeneity upper bound[c]		−.429
			(.012)
4	Placebo kink at:		
(a)		$401	.069
			(.055)
(b)		$450	.096
			(.068)
(c)		$500	.000
			(.005)
(d)		$550	−.056
			(.027)

Notes: The estimates use the percentile-based estimator: equation (27) with $p_t = 1/2$ and $p_1 = 1$. The range of donations Θ = [$201, $1,000]; using other ranges produces similar results. Bootstrapped standard errors are in parentheses.

[a] The baseline estimate is formed by pooling donors in the Θ range from all control states.

[b] The heterogeneity lower bound is formed by pooling donors in the Θ range separately in each control state that has at least 100 observations, and then selecting the control state whose marginal donor gives largest amount (see Section 3D).

[c] The heterogeneity upper bound is formed as in the previous note, except that the control state selected is the one whose marginal donor gives smallest amount (see Section 3D).

7. Appendix

The Appendix is supplemental, and not intended for in-print publication. It will be available on-line, or from the authors upon request.

There are several sections. The first three provide theoretical derivations. Section A.1 derives equations (7) and (8), as well as the income effects q_1 and q_2. Section A.2 derives Slutsky decompositions. Section A.3 re-derives the Section A.1 price and income effects for more general preferences $U(c_i,\ g_i,\ R_i,\ R_{-i})$.

Section A.4 shows that estimates based on a state tax credit are unaffected by federal deductibility. Section A.5 dervies the baseline bunching formula. Section A.6 describes how our empirical results compare to nearest neighbor bandwidth results as in Saez (2010). Section A.7 describes an example of estimating uncompensated elasticities using placebo kinks.

Section A.8 contains tables with further estimation results.

A.1. Derivations of price and income effects.

In this section we derive the tax-price and match-price effects given in (7) and (8). Begin with the first order condition:

$$- p_m p_t U_c(y_i - \tau_i - p_m p_t R + p_m p_t \lambda R_{-i},\ p_m(R - \lambda\ R_{-i}),\ R) + p_m U_g + U_R = 0. \tag{28}$$

Differentiating this with respect to R yields the second order condition:

$$\begin{aligned} -p_m p_t U_{cc}\Big[-p_m\ p_t\Big] - {p_m}^2 p_t U_{cg} - p_m p_t U_{cR} \\ +p_m U_{gc}\Big[-p_m\ p_t\Big] + {p_m}^2 U_{gg} + p_m U_{gR} \\ +U_{Rc}\Big[-p_m\ p_t\Big] + p_m U_{Rg} + U_{RR}. \end{aligned} \tag{29}$$

which we assume is negative. Solving the first order condition for R produces, as defined in the text in (6),

$$R^* = q(y_i - \tau_i + p_m p_t \lambda R_{-i},\ \lambda p_m R_{-i},\ p_m p_t,\ p_m). \tag{30}$$

We will use (28), (29), and (30) to construct the partial derivative q_1. Suppose all else equal y_i increases. Fully differentiating the first order condition yields:

$$\begin{aligned}
-p_m p_t U_{cc}\left[1 - p_m\ p_t\ \frac{\partial R}{\partial y_i}\right] - {p_m}^2 p_t U_{cg}\frac{\partial R}{\partial y_i} - p_m p_t U_{cR}\frac{\partial R}{\partial y_i} \\
+p_m U_{gc}\left[1 - p_m\ p_t\frac{\partial R}{\partial y_i}\right] + {p_m}^2 U_{gg}\frac{\partial R}{\partial y_i} + p_m U_{gR}\frac{\partial R}{\partial y_i} \\
+U_{Rc}\left[1 - p_m\ p_t\frac{\partial R}{\partial y_i}\right] + p_m U_{Rg}\frac{\partial R}{\partial y_i} + U_{RR}\frac{\partial R}{\partial y_i}.
\end{aligned} \tag{31}$$

Collecting terms together, we can express this as:

$$-p_m p_t U_{cc} + p_m U_{gc} + U_{Rc} \quad = \quad \frac{\partial R}{\partial y_i}\Delta \tag{32}$$

where as in the text Δ is the negative of the second order condition (and thus $\Delta > 0$); specifically $\Delta = -p_t^2 p_m^2 U_{cc} + 2p_t p_m^2 U_{cg} + 2p_t p_m U_{cR} - 2p_m U_{gR} - p_m^2 U_{gg} - U_{RR}$. Noting that $q_1 = \frac{\partial R}{\partial y_i}$, it follows from (32) that

$$q_1 \quad = \quad \frac{-p_m p_t U_{cc} + p_m U_{gc} + U_{Rc}}{\Delta}. \tag{33}$$

Continuing this process for q_2, totally differentiate the first order condition with respect to R_{-i}:

$$-p_m p_t U_{cc}\Big[p_m p_t\lambda - p_m\ p_t\ \frac{\partial R}{\partial R_{-i}}\Big] - {p_m}^2 p_t U_{cg}\Big[-\lambda + \frac{\partial R}{\partial R_{-i}}\Big] - p_m p_t U_{cR}\frac{\partial R}{\partial R_{-i}}$$
$$+p_m U_{gc}\Big[p_m p_t\lambda - p_m\ p_t\frac{\partial R}{\partial R_{-i}}\Big] + {p_m}^2 U_{gg}\Big[-\lambda + \frac{\partial R}{\partial R_{-i}}\Big] + p_m U_{gR}\frac{\partial R}{\partial R_{-i}}$$
$$+U_{Rc}\Big[p_m p_t\lambda - p_m\ p_t\frac{\partial R}{\partial R_{-i}}\Big] + p_m U_{Rg}\Big[-\lambda + \frac{\partial R}{\partial R_{-i}}\Big] + U_{RR}\frac{\partial R}{\partial R_{-i}}. \tag{34}$$

Gathering terms,

$$p_m p_t \lambda q_1 + \frac{\lambda({p_m}^2 p_t U_{gc} - {p_m}^2 U_{gg} - p_m U_{Rg})}{\Delta} \ = \ \frac{\partial R}{\partial R_{-i}} \tag{35}$$

and noting from (30) that $\frac{\partial R}{\partial R_{-i}} = \lambda p_m(p_t q_1 + q_2)$, equation (35) yields:

$$q_2 \ = \ \frac{p_m p_t U_{gc} - p_m U_{gg} - U_{Rg}}{\Delta}. \tag{36}$$

With derivations for q_1 and q_2 in hand, we proceed to deriving (7) and (8) in the same fashion. Totally differentiate the first order condition in (28) with respect to p_t:

$$-p_m U_c - p_m p_t U_{cc}\Big[-p_m(R-\lambda R_{-i}) - p_m\ p_t\ \frac{\partial R}{\partial p_t}\Big] - {p_m}^2 p_t U_{cg}\frac{\partial R}{\partial p_t} - p_m p_t U_{cR}\frac{\partial R}{\partial p_t}$$
$$+p_m U_{gc}\Big[-p_m(R-\lambda R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_t}\Big] + {p_m}^2 U_{gg}\frac{\partial R}{\partial p_t} + p_m U_{gR}\frac{\partial R}{\partial p_t}$$
$$+U_{Rc}\Big[-p_m(R-\lambda R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_t}\Big] + p_m U_{Rg}\frac{\partial R}{\partial p_t} + U_{RR}\frac{\partial R}{\partial p_t}. \tag{37}$$

Gathering terms, and noting that $R_i = R - \lambda R_{-i}$, yields equation (7):

$$\frac{\partial R}{\partial p_t} \ = \ \frac{-p_m\ U_c}{\Delta} \ - \ q_1\ p_m\ R_i. \tag{38}$$

Totally differentiating the first order condition in (28) with respect to p_m, we have:

$$-p_t U_c - p_m p_t U_{cc}\Big[-p_t(R-\lambda R_{-i}) - p_m p_t\frac{\partial R}{\partial p_m}\Big] - p_m p_t U_{cg}\Big[R-\lambda R_{-i} + p_m\frac{\partial R}{\partial p_m}\Big] - p_m p_t U_{cR}\frac{\partial R}{\partial p_m}$$

$$+U_g + p_m U_{gc}\Big[-p_t(R-\lambda R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_m}\Big] + p_m U_{gg}\Big[R-\lambda R_{-i} + p_m\frac{\partial R}{\partial p_m}\Big] + p_m U_{gR}\frac{\partial R}{\partial p_m}$$

$$+U_{Rc}\Big[-p_t(R-\lambda R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_m}\Big] + U_{Rg}\Big[R-\lambda R_{-i} + p_m\frac{\partial R}{\partial p_m}\Big] + U_{RR}\frac{\partial R}{\partial p_m}.$$

Setting this equal to zero, using the above derivations for q_1 and q_2, and again noting $R_i = R - \lambda R_{-i}$ produces (8):

$$\frac{\partial R}{\partial p_m} = \frac{-p_t\ U_c}{\Delta} - q_1\ p_t\ R_i + \frac{U_g}{\Delta} - q_2\ R_i. \tag{39}$$

A.2 Slutsky decompositions for e_t and e_m.

A.2.a Decomposition with respect to the tax-price.

We begin with the tax-price and the compensated demand response $\frac{\partial R^H}{\partial p_t}$. Consider minimizing $c_i + p_t p_m R$ subject to $U(c_i, g_i, R) = u^0$. As before we will optimize over R, where $R = R_i + \lambda R_{-i}$ and $g_i = p_m(R - \lambda R_{-i})$. (Of course, we cannot use the budget constraint to substitute in for c_i since there is no budget constraint for this problem.) We thus minimize:

$$\mathcal{L} = c_i + p_t p_m R + \mu(u^0 - U(c_i,\ p_m(R - \lambda R_{-i}),\ R)) \tag{40}$$

where μ is a Lagrange multiplier. The first order conditions are:

$$1 - \mu U_c = 0$$

$$p_t p_m - \mu(p_m U_g + U_R) = 0$$

$$U(c_i,\ p_m(R - \lambda R_{-i}),\ R)) = u^0 \tag{41}$$

which describe the solutions for compensated demand R^H and $R_i^H = R^H - \lambda R_{-i}$, where of course λR_{-i} is exogenously given. Combining the first two equations in (41) yields

$$-p_t p_m U_c(c_i,\ p_m(R - \lambda R_{-i}),\ R) + p_m U_g + U_R = 0 \tag{42}$$

Totally differentiate this with respect to p_t:

$$\begin{aligned} -p_m U_c - p_m p_t U_{cc}\frac{\partial c^H}{\partial p_t} - {p_m}^2 p_t U_{cg}\frac{\partial R^H}{\partial p_t} - p_m p_t U_{cR}\frac{\partial R^H}{\partial p_t} \\ +p_m U_{gc}\frac{\partial c^H}{\partial p_t} + {p_m}^2 U_{gg}\frac{\partial R^H}{\partial p_t} + p_m U_{gR}\frac{\partial R^H}{\partial p_t} \\ +U_{Rc}\frac{\partial c^H}{\partial p_t} + p_m U_{Rg}\frac{\partial R^H}{\partial p_t} + U_{RR}\frac{\partial R^H}{\partial p_t}. \end{aligned} \tag{43}$$

Next, totally differentiate the last equation in (41) with respect to p_t:

$$U_c\frac{\partial c^H}{\partial p_t} + p_m U_g\frac{\partial R^H}{\partial p_t} + U_R\frac{\partial R^H}{\partial p_t} = du^0 \tag{44}$$

By (44), we have that $\frac{\partial c^H}{\partial p_t} = \frac{1}{U_c}(du^0 - (p_m U_g + U_R)\frac{\partial R^H}{\partial p_t})$. For compensated demand, $du^0 = 0$. By (42), we have that $(p_m U_g + U_R)/U_c = p_t p_m$, so that $\frac{\partial c^H}{\partial p_t} = -p_t p_m \frac{\partial R^H}{\partial p_t}$. Plugging this into (43) yields:

$$-p_m U_c \ = \ \Delta\frac{\partial R^H}{\partial p_t} \tag{45}$$

where as before $\Delta = -p_t^2 p_m^2 U_{cc} + 2p_t p_m^2 U_{cg} + 2p_t p_m U_{cR} - 2p_m U_{gR} - p_m^2 U_{gg} - U_{RR}$. Recall $R_i^H = R^H - \lambda R_{-i}$, and the last term is by definition exogenous, so $\frac{\partial R_i^H}{\partial p_t} = \frac{\partial R^H}{\partial p_t} = \frac{-p_m U_c}{\Delta}$. Therefore,

$$\frac{\partial R_i^H}{\partial p_t}\frac{p_t}{R_i} = \frac{-p_m\ p_t\ U_c}{R_i\ \Delta} \tag{46}$$

which is equation (11) in the text.

And lastly, the uncompensated price effect given in (7) and derived in equation (38) is $\frac{\partial R}{\partial p_t} = \frac{-p_m\ U_c}{\Delta} - p_m q_1 R_i$. Putting this together with equation (46), we can write

$$e_t = \left(\frac{-p_m\ U_c}{\Delta} - p_m q_1 R_i\right)\frac{p_t}{R_i} \ = \ \frac{\partial R_i^H}{\partial p_t}\frac{p_t}{R_i} - \frac{p_m p_t R_i}{y_i} q_1 \frac{y_i}{R_i} = e_t^H - b_{R_i} e_y \tag{47}$$

where $b_{R_i} \equiv \frac{p_m p_t R_i}{y_i}$ and equation (12) is derived.

A.2.b Decomposition with respect to the match-price.

The derivation for the match-price is similar. Totally differentiate (42) with respect to p_m:

$$\begin{aligned}-p_t U_c - p_m p_t U_{cc}\frac{\partial c^H}{\partial p_m} - p_m p_t U_{cg}\Big[p_m\frac{\partial R^H}{\partial p_m} + (R - \lambda R_{-i})\Big] - p_m p_t U_{cR}\frac{\partial R^H}{\partial p_m}\\ U_g + p_m U_{gc}\frac{\partial c^H}{\partial p_m} + p_m U_{gg}\Big[p_m\frac{\partial R^H}{\partial p_m} + (R - \lambda R_{-i})\Big] + p_m U_{gR}\frac{\partial R^H}{\partial p_m}\\ +U_{Rc}\frac{\partial c^H}{\partial p_m} + U_{Rg}\Big[p_m\frac{\partial R^H}{\partial p_m} + (R - \lambda R_{-i})\Big] + U_{RR}\frac{\partial R^H}{\partial p_m}.\end{aligned} \tag{48}$$

By differentiating the last equation in (41), we have $\frac{\partial c^H}{\partial p_m} = \frac{1}{U_c}(du^0 - (p_m U_g + U_R)\frac{\partial R^H}{\partial p_m} - (R - \lambda R_{-i})U_g)$. This is crucially different than before because of the last term on the right hand side. We can simplify this expression noting that (a) $R - \lambda R_{-i} = R_i$ (b) once again $du^0 = 0$ and (c) using the first order condition to simplify $(p_m U_g + U_R)/U_c$ as before. This produces $\frac{\partial c^H}{\partial p_m} = -p_t p_m \frac{\partial R^H}{\partial p_m} - R_i \frac{U_g}{U_c}$.

Plugging this into (48) and rearranging yields:

$$\frac{\partial R^H}{\partial p_m} = \frac{-p_t U_c + U_g}{\Delta} - \frac{p_t p_m U_{cg} - p_m U_{gg} - U_{Rg}}{\Delta} R_i - \frac{-p_t p_m U_{cc} + p_m U_{gc} + U_{Rc}}{\Delta} \frac{U_g}{U_c} R_i \quad (49)$$

Using equations (33) and (36), this becomes:

$$\frac{\partial R^H}{\partial p_m} = \frac{-p_t U_c + U_g}{\Delta} - q_2 R_i - q_1 \frac{U_g}{U_c} R_i. \quad (50)$$

Equation (50) can be used to re-write the text's equation (8)—reproduced here: $\frac{\partial R}{\partial p_m} = \frac{-p_t\ U_c}{\Delta} - q_1\ p_t\ R_i + \frac{U_g}{\Delta} - q_2\ R_i$ — as: $\frac{\partial R}{\partial p_m} = \frac{\partial R^H}{\partial p_m} - (p_t - \frac{U_g}{U_c}) q_1 R_i$. Converting this to elasticity form (using as above $\frac{\partial R^H}{\partial p_m} = \frac{\partial R_i^H}{\partial p_m}$ and as in the text the uncompensated version $\frac{\partial R}{\partial p_m} = \frac{\partial R_i}{\partial p_m}$), yields:

$$\begin{aligned} e_m &= e_m^H - \left[p_m\left(p_t - \frac{U_g}{U_c}\right)\frac{R_i}{y_i}\right] \left(q_1\ \frac{y_i}{R_i}\right) \\ &= e_m^H - \tilde{b}_{R_i}\ e_y \end{aligned} \quad (51)$$

where $\tilde{b}_{R_i} = \tilde{p} R_i / y_i$ and $\tilde{p} \equiv p_m(p_t - \frac{U_g}{U_c})$. This is equation (13) in the text.

As an aside, convert equation (50) into elasticity form:

$$\begin{aligned} e_m^H &= \frac{-p_m\ p_t\ U_c}{\Delta\ R_i} + \frac{p_m U_g}{\Delta\ R_i} - p_m\left(\frac{U_g}{U_c}\ q_1 + q_2\right) \\ &= e_t^H + \frac{p_m U_g}{\Delta\ R_i} - p_m\left(\frac{U_g}{U_c}\ q_1 + q_2\right) \end{aligned} \quad (52)$$

where the second line uses equation (46) (equation (11) from the text). From equation (52) it is straightforward to see that the match-price compensated demand also depends on the unobserved $\frac{U_g}{U_c}$ as noted in footnote 3 in the text.

A.3 Price effects with preferences $U(c_i,\ g_i,\ R_i,\ R_{-i})$.

In this section we produce results for the utility function $U(c_i,\ g_i,\ R_i,\ R_{-i})$. These results do not change the prior intuition that tax- and match- price effects can differ, or the relevant tests for different models of giving, or the normative result that changes in tax prices are preferred.

Define total observable impact as $R = R_i + R_{-i}$. Write the new utility function as a function of R:

$$U(y_i - \tau_i - p_m p_t R + p_m p_t R_{-i},\ p_m(R - R_{-i}),\ R - R_{-i},\ R_{-i}). \tag{53}$$

Since the last term in the utility function is taken as given, the first order condition resembles the one given in the text:

$$-p_t p_m U_c(y_i - \tau_i - p_m p_t R + p_m p_t R_{-i}, p_m(R - R_{-i}), R - R_{-i}, R_{-i}) + p_m U_g + U_{R_i} = 0. \tag{54}$$

And the solution can be expressed as

$$R^* = q(y_i - \tau_i + p_m p_t R_{-i},\ p_m R_{-i},\ p_m p_t,\ p_m). \tag{55}$$

which upon first inspection actually looks simpler than the q function used in the text, as there is no unobservable λ parameter.

Differentiating (54) with respect to R yields the second order condition, which again essentially matches the one used earlier:

$$\begin{aligned} &-p_m p_t U_{cc}\Big[-p_m\ p_t\Big] - {p_m}^2 p_t U_{cg} - p_m p_t U_{cR_i} \\ &+p_m U_{gc}\Big[-p_m\ p_t\Big] + {p_m}^2 U_{gg} + p_m U_{gR_i} \\ &+U_{R_i c}\Big[-p_m\ p_t\Big] + p_m U_{Rg} + U_{R_i R_i}. \end{aligned} \tag{56}$$

which we again assume is negative.

Differentiating with respect to y_i yields

$$
\begin{aligned}
-p_m p_t U_{cc}\Big[1 - p_m\ p_t\ \frac{\partial R}{\partial y_i}\Big] - {p_m}^2 p_t U_{cg}\frac{\partial R}{\partial y_i} - p_m p_t U_{cR_i}\frac{\partial R}{\partial y_i} \\
+p_m U_{gc}\Big[1 - p_m\ p_t\frac{\partial R}{\partial y_i}\Big] + {p_m}^2 U_{gg}\frac{\partial R}{\partial y_i} + p_m U_{gR_i}\frac{\partial R}{\partial y_i} \\
+U_{R_i c}\Big[1 - p_m\ p_t\frac{\partial R}{\partial y_i}\Big] + p_m U_{Rg}\frac{\partial R}{\partial y_i} + U_{R_i R_i}\frac{\partial R}{\partial y_i}.
\end{aligned}
\tag{57}
$$

And the resulting derivation is the same as before:

$$
q_1 \quad = \quad \frac{-p_m p_t U_{cc} + p_m U_{gc} + U_{R_i c}}{\Delta}. \tag{58}
$$

With this in hand, it is straightforward to derive the response for the tax-price:

$$
\begin{aligned}
-p_m U_c - p_m p_t U_{cc}\Big[- p_m(R - R_{-i}) - p_m\ p_t\ \frac{\partial R}{\partial p_t}\Big] - {p_m}^2 p_t U_{cg}\frac{\partial R}{\partial p_t} - p_m p_t U_{cR}\frac{\partial R}{\partial p_t} \\
+p_m U_{gc}\Big[- p_m(R - R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_t}\Big] + {p_m}^2 U_{gg}\frac{\partial R}{\partial p_t} + p_m U_{gR}\frac{\partial R}{\partial p_t} \\
+U_{Rc}\Big[- p_m(R - R_{-i}) - p_m\ p_t\frac{\partial R}{\partial p_t}\Big] + p_m U_{Rg}\frac{\partial R}{\partial p_t} + U_{RR}\frac{\partial R}{\partial p_t}
\end{aligned}
\tag{59}
$$

producing as before:

$$
\frac{\partial R}{\partial p_t} \quad = \quad \frac{-p_m\ U_c}{\Delta} \ - \ q_1\ p_m\ R_i. \tag{60}
$$

Differentiating the first order condition in (54) with respect to p_m, we now have:

$$-p_t U_c - p_m p_t U_{cc}\Big[-p_t(R-R_{-i}) - p_m p_t \frac{\partial R}{\partial p_m}\Big] - p_m p_t U_{cg}\Big[R - R_{-i} + p_m \frac{\partial R}{\partial p_m}\Big] - p_m p_t U_{cR_i}\frac{\partial R}{\partial p_m}$$

$$+U_g + p_m U_{gc}\Big[-p_t(R-R_{-i}) - p_m\ p_t \frac{\partial R}{\partial p_m}\Big] + p_m U_{gg}\Big[R - R_{-i} + p_m \frac{\partial R}{\partial p_m}\Big] + p_m U_{gR_i}\frac{\partial R}{\partial p_m}$$

$$+U_{R_i c}\Big[-p_t(R-R_{-i}) - p_m\ p_t \frac{\partial R}{\partial p_m}\Big] + U_{Rg}\Big[R - R_{-i} + p_m \frac{\partial R}{\partial p_m}\Big] + U_{R_i R_i}\frac{\partial R}{\partial p_m}$$

so that:

$$\frac{\partial R}{\partial p_m} \;=\; \frac{-p_t\ U_c}{\Delta} - q_1\ p_t\ R_i \;+\; \frac{U_g}{\Delta} - \tilde{q_2}\ R_i \qquad (61)$$

where just as before

$$\tilde{q_2} = \frac{p_m p_t U_{cg} - p_m U_{gg} - U_{R_i g}}{\Delta} \qquad (62)$$

The expressions governing tax-price and match-price responses are thus the same as those given with in the main text.

A.4 Elasticity estimates based on a state tax credit are unaffected by federal deductibility.

This section shows that the inclusion of the deductibility of gifts from federal taxable income does not alter the elasticity estimate from a calculation based on a state-income tax credit. Suppose the state *tax rate* is t_s and the state *tax credit* is c_s. The budget constraint is then $c_i + g_i = y_i - y_i t_s + g_i c_s$. State taxes paid are $y_i t_s - g_i c_s$.

Now suppose the federal government taxes income at rate t_f, but that both giving and state taxes are deductible. Then the budget constraint is $c_i + g_i = y_i - y_i t_s + g_i c_s - (y - y_i t_s + g_i c_s - g_i) t_f = y_i(1 - t_s - t_f + t_s t_f) + g_i(c_s + t_f - c_s t_f)$.

Denote the combined federal-state price of giving as $p_{fs} = 1 - c_s - t_f + c_s t_f$. The "$c_s t_f$" appears because state income tax paid is deductible from federal taxable income. The federal-state price can be re-written as $p_{fs} = (1 - c_s)\ (1 - t_f) = p_s\ p_f$, where $p_s \equiv 1 - c_s$ is defined as the state price (in isolation from the federal price), and $p_f \equiv 1 - t_f$ is defined as the federal price (in isolation from the state price).

Obviously, $\ln(p_{fs}) = \ln(p_s) + \ln(p_f)$ and $\frac{d\ln\ p_{fs}}{d\ln\ p_s} = 1$. It then follows from the chain rule that elasticities with respect to p_{fs} and p_s are identical: $\frac{d\ \ln g_i}{d\ \ln p_s} = \frac{d\ \ln g_i}{d\ \ln p_{fs}} \frac{d\ \ln p_{fs}}{d\ \ln p_s} = \frac{d\ \ln g_i}{d\ \ln p_{fs}}$.

A.5 Derivation of baseline bunching formula.

This derivation follows Saez (2010). Preferences are given by:

$$U = x + \frac{\theta}{1+1/e}\left(\frac{R}{\theta}\right)^{1+1/e}$$

and are maximized subject to $x + pR = Y - \tau$, where p is the price of giving. The optimal choice of giving is then $R = \theta\ p^e$, where the price elasticity $e < 0$.

Note that θ is a preference parameter that indexes generosity; individuals with a higher value of θ will give larger amounts. Suppose that the price of giving is initially p_0 and it is then raised to a higher price p_1 above R_k. Consider individuals initially at an interior solution above R_k when facing p_0; those individuals with θ values between $(R_k/p_0^e,\ R_k/p_1^e)$ would choose an optimum above R_k when the price is low and below R_k when the price is high: they will bunch. Note the "marginal buncher" on the high end, with $\theta = R_k/\ p_1^e$, would in a world with low prices p_0 choose $R = R_k\ (p_0\ /\ p_1)^e$. The low-end marginal buncher would choose R_k. Thus, the range of bunching is $R_b - R_a = R_k\left(\frac{p_0^e}{p_1^e} - 1\right)$ where R_b and R_a are taken from Figure 1.

Let $h_0(R)$ be the density of giving when p_0 applies to all levels of giving; e.g., $p_0 = (1 - t)$ and the cap at R = R_k is counterfactually removed. Let $H_0(R)$ be the corresponding cumulative distribution function. Denote giving in this counterfactual as R_0. Then $R_0 = \theta p_0^e \rightarrow \theta = R_0/p_0^e$, and the counterfactual $h_0(\mathrm{R}) = f(\mathrm{R}/p_0^e)\frac{1}{p_0^e}$, where $f()$ is the density of θ. This follows since $H_0(R) = P[\theta p_0^{-e} < R] = F(\mathrm{R}p_0^e)$, where F is the cdf of θ, and then differentiating by R.

Let $h(R)$ be the density of giving we observe. Over the range $R < R_k$ below the kink the observed density $h(R)$ corresponds to $h_0(R)$. But for $R > R_k$ the observed density $h(R)$ is not $h_0(R)$. Giving over the range $R > R_k$ is $R = \theta p_1^e$. This can be rewritten $R = R_0 p_1^e / p_0^e$, where R_0 is the counterfactual amount of giving for $R > R_k$ that would be observed if the cap at $R = R_k$ was removed. Therefore, the observed density of R for $R > R_k$ can be expressed in terms of the counterfactual $h_0(R)$: $h(R) = h_0(R\ p_0^e /\ p_1^e)\ p_0^e\ /\ p_1^e$.

Define $h_{R_k^-}$ to be the limit of the observed density $h(R)$ as R approaches R_k from below, and define $h_{R_k^+}$ to be the limit as R approaches R_k from above. Then $h_{R_k^-} = h_0(R_k)$. The limit from above is $h_{R_k^+} = h_0(R_k p_0^e/p_1^e) p_0^e/p_1^e$, implying that the limit of the counterfactual density from above is $h_{R_k^+} p_1^e/p_0^e$; $(p_1^e\ /p_0^e)$ is the adjustment to the observed density, to get the counterfactual density, discussed in Section 2A. Using a trapezoidal approximation to the integral as in Saez (2010), the amount of bunching β at the kink can be expressed as a function of observables and the counterfactual density of giving. Then using the relationships just described, β can be expressed in terms of the observed density of giving:

$$\beta = \int_{R_k}^{R_k\left(\frac{p_0^e}{p_1^e}\right)} h_0\left(R\right)dR \cong \frac{h_0\left(R_k\right) + h_0\left(R_k p_0^e/p_1^e\right)}{2} R_k \left(\frac{p_0^e}{p_1^e} - 1\right) = \frac{h_{R_k^-} + h_{R_k^+}\left(\frac{p_1^e}{p_0^e}\right)}{2} R_k \left(\frac{p_0^e}{p_1^e} - 1\right)$$

which is equation (23) in Section 3C.

A.6 Nearest neighbor bandwidth compared to the bandwidth in Saez (2010).

As noted in Section 3C, our use of three bins of equal width w in the nearest neighbor method differs slightly from the bandwidths used in Saez' (2010) original method. In the original method, if the bandwidth is w the mass point around the kink is defined to be the amounts falling in the interval (\$400 $-$ w, \$400 + w) and the counterfactual density below and above the kink is estimated using the amounts in the respective intervals [\$400 $-$ $2w$,

\$400 − w] and [\$400 + w, \$400 + $2w$]. Accordingly, bunching at the kink is estimated as the fraction in the $2w$-wide interval around the kink minus the sum of the fractions in the intervals below and above the kink, each of which are w-wide.

In our implementation (Table 4, rows 2 and 3) the interval around the kink is (\$400 − w, \$400 + w), the counterfactual density below and above the kink is estimated using the intervals (\$400 – 3/2 w, \$400 − w] and [\$400 + w, \$400 + 3/2 w), and bunching at the kink is estimated according to (24): the fraction in the w-wide interval around the kink minus the average of the fractions in the w-wide intervals below and above the kink. Using equal width bins for nearest neighbor established the same definition of "bandwidth" across the three methods of Section 3C—nearest neighbor, polynomial, and nearest round neighbor.

Redoing the Table 4 nearest neighbor estimates in rows 2 and 3 but implementing the nearest neighbor estimator exactly as in Saez (2010) with bandwidths of \$12.50 (matching row 2 in the table) and \$25 (matching row 3 in the table) produces similar but slightly smaller estimates of −.283 (.026) and −.116 (.014), respectively.

A.7 Placebo kinks for the uncompensated elasticity estimates: An example.

Consider quasilinear preferences, where the optimal choice of giving is $R = \theta p^e$ for $e < 0$. As elsewhere, let the low price of giving created by the credit be p_0 and the higher post credit price be p_1. Outside of Indiana, where there is no credit, the price of giving is always p_1. The kink level of giving is denoted R_k. In Indiana, individuals with $\theta < R_k p_0^{-e}$ will choose giving levels below the kink, those with $\theta > R_k p_1^{-e}$ will choose giving levels above the kink, and those with $\theta \in [R_k p_0^{-e},\ R_k p_1^{-e}]$ will bunch. For both Indiana and the control states, let F be the distribution function of θ defined over Θ.

Consider first the true kink estimator $\hat{e}_u$. Here, the person in Indiana giving just below the kink value R_k will have a $\theta \cong R_k p_0^{-e}$, and their percentile value will then be $\rho = F(R_k p_0^{-e})$. In a control state, the donor with this θ will again have percentile value ρ in the distribution of giving, but they face price p_1. Hence, their level of giving will be $R(\rho)=\theta p_1^e = R_k\left(\frac{p_1}{p_0}\right)^e$.

Recall that equation (27), reproduced here, uses an arc-elasticity formula:

$$\hat{e}_u = -\frac{(R_k - R(\rho))/((R_k + R(\rho))/2)}{(p_1 - p_0)/((p_1 + p_0)/2)}.$$

It is straightforward to verify that applying $R(\rho)=R_k\left(\frac{p_1}{p_0}\right)^e$ to equation (27) will yield, for reasonably small values of e (such as between -2 and zero), a result very close to e.

Now consider a placebo kink placed above R_k: denote this placebo kink $\widetilde{R} > R_k$. For the person in Indiana giving this amount, we have $\theta = \widetilde{R}p_1^{-e}$ and $\rho = F(\widetilde{R}p_1^{-e})$. In the control states the person at this percentile of giving will have the same θ value *and* will face the same price as the Indiana giver. They thus will have the same level of giving: $R(\rho) = \theta p_1^e = \widetilde{R}\ p_1^{-e} p_1^e = \widetilde{R}$. Applying this to the equation (27) will thus yield an elasticity of zero for any giving value above the kink.

The situation is different for a placebo kink placed below R_k, because individuals in the control states with at giving levels below g* face a different price than they do in Indiana. Denote the placebo kink $\check{R} < R_k$. Then for someone giving this level in Indiana $\theta = \check{R}p_0^{-e}$ and $\rho = F(\check{R}p_0^{-e})$. In a control state, the person at this percentile of giving has $\theta = \check{R}p_0^{-e}$ and giving level $\check{R}(\rho) = \theta p_1^e = \check{R}\left(\frac{p_1}{p_0}\right)^e$. Plugging this into equation (27) will *not* produce the elasticity estimate e. However, as the placebo kink approaches the true kink $\check{R} \longrightarrow R_k$ from below, the estimator will approach the true elasticity value.

In short, under the identifying assumptions, an individual at a given percentile value of giving in Indiana will have the same θ as the person at this percentile of giving in the control states. *Above* the kink, these two individuals not only have the same taste for giving but they also face the same price. All else equal, the person in Indiana above the kink will be \$200 richer (because of the credit), but if income effects are negligible (or zero, as they are in the present example) their choice of giving will be the same and a placebo kink should return a zero estimate. *Below* the kink, an individual at a certain percentile of the giving distribution in Indiana has the same θ as a person at this percentile in the control state distribution, but they face *different* prices, so that their level of giving will be different. However, as the placebo kink approaches the true kink, the difference in their giving will approximate a difference in giving that can recover the true elasticity.

A.8 Further estimation results.

This section contains the supplemental tables.

Table AT1. Match-price elasticity: Difference-in-differences estimates using the number of donations (extensive margin).

	Baseline	Month by year dummies	State dummies	State by year dummies	Class trends
Matching treatment for the 1960 class (δ)	-.119	-0.098	-0.102	-0.118	-.131
	(.083)	(.081)	(.097)	(.097)	(.092)
Implied elasticity of the number of donations	.0593	.049	.051	.059	.065
	(.125)	(.122)	(.146)	(.146)	(.138)
Matching treatment placebo for the other classes[a]	Yes	Yes	Yes	Yes	Yes
25^{th}, 50^{th}, 75^{th} graduation anniversary dummies	Yes	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Class dummies	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	Yes	Yes	No
Month by year dummies	No	Yes	Yes	Yes	No
State by year dummies	No	No	No	Yes	No
Class trends	No	No	No	No	Yes

Notes: The dependent variable is the total number of donations by each class in a state, month, and year. There are 156,589 class x state x month x year cells. The first row presents the estimates on a dummy that equals one for the 1960 class during the time period of the match; standard errors are clustered by graduating class cohort (in parentheses). The second row converts the row 1 coefficients into elasticity estimates using equation (22) with p_m = 1/2 and p_1 = 1 and is calculated relative to the sample mean number of donations; delta method standard errors are in parentheses.

[a] A dummy that equals one for the other classes during the 19 month period around their 50^{th} anniversaries.

Table AT2. Rebate-price elasticity: Compensated elasticity – Additional nearest round neighbor estimates.

Row	Sub-sample	Lower bound estimate	Point estimate (linear interpolation)	Upper bound estimate
1	Baseline (from Table 4)	−.121	−.197	−.293
		(.021)	(.024)	(.033)
2	Lottery-ineligible[a]	−.223	−.231	−.242
		(.051)	(.051)	(.063)
3	2004 – 2006[b]	−.074	−.171	−.293
		(.042)	(.045)	(.063)
4	2007 – 2015[b]	−.133	−.204	−.293
		(.023)	(.024)	(.032)
5	Number of years giving = 1 to 5[c]	.108	−.058	−.438
		(.100)	(.143)	(.276)
6	Number of years giving = 6 to 12[c]	−.152	−.211	−.281
		(.023)	(.023)	(.028)
7	Number of years giving = 1 to 5[d]	−.118	−.191	−.338
		(.113)	(.176)	(.408)
8	Number of years giving = 6 to 12[d]	−.121	−.197	−.289
		(.024)	(.025)	(.031)

Notes. Mass points at $300 and $500 used to identify counterfactual bunching at the kink. The lower bound is based on the mass point at $300, the upper bound is based on the mass point at $500, and the point estimate is based on linear interpolation between the mass points. Donation amounts are placed in bins of width $25. One bin below and one bin above the kink used to estimate the counterfactual density. Bootstrapped standard errors are in parentheses. The estimates use equation (23) where $p_t = 1/2$ and $p_1 = 1$.

[a] This estimate uses the sub-sample not made eligible for the football lottery with a donation of $200 (e.g., recent alumni, senior alumni, and non-alumni).
[b] During 2004-2006 most alumni became eligible for the football lottery with a smaller, $100, donation. During 2007-2015 the necessary donation was $200.
[c] The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the only the years the person gave a donation designated as joint.
[d] The split of the sample into number of years giving “1 to 5” and “6 to 12” uses the years the person gave a donation designated non-joint as well as years in which the person gave as joint. However, the years used in the estimation of the elasticities are only those in which the person gave as joint.

Table AT3. Rebate-price elasticity: Uncompensated elasticity – Additional percentile-based estimates.

Row	Method	Estimate
1	Baseline (from Table 6)	−.265
		(.042)
2	Lottery-ineligible	−.31
		(.062)
3	Different ranges of donations	
(a)	Donation range: $100 - 1,000	−.38
		(.061)
(b)	Donation range: $200 - 1,000	−.31
		(.047)
(c)	Donation range: $300 - 1,000	−.20
		(.026)
(d)	Donation range: $201 - 500	−.27
		(.04)
(e)	Donation range: $201 - 5,000	−.31
		(.05)

Notes: The estimates use the percentile-based estimator: equation (27) with $p_t = 1/2$ and $p_1 = 1$. In row 2 the range of donations Θ = [$201, $1,000] (as it is in row 1), but the sub-sample is restricted to people not made eligible for the football lottery with a donation of $200 (e.g., recent alumni, senior alumni, and non-alumni). Row 3 uses the full sample, but varies the range of donations Θ as indicated. Bootstrapped standard errors are in parentheses.